THE UNIVERSITY *of* EDINBURGH

The Cambridge School.

The life, work and influence of James Ward, W.H.R. Rivers, C.S. Myers and Sir Frederic Bartlett.

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Ph.D.

University of Edinburgh

1978

ACKNOWLEDGEMENTS

Most thanks are due to the Social Science Research Council, who provided the financial support, and my supervisor Dr. John Beloff, who provided the intellectual support, for this thesis. The Ministry of Defence also supported this work in its later stages.

Many thanks are also due to Professor O.L. Zangwill and Dr. John Mollon of the Psychological Laboratory, Cambridge, for guiding me to the appropriate archives and also for arranging to supply me with many of the photographs which appear in the text. Plates 1, 4, 7, 8, 12, 13, 14, 15, 16, 17 and 18 appear by kind permission of Professor Zangwill, as do all quotations from material held in the Cambridge Laboratory archives. Plate 2 appears by courtesy of Trinity College, Cambridge.

Several people consented to be interviewed by me and between them supplied invaluable ideas, background material and personal recollections. Thus my thanks are due to: Dr. Alice Heim, Dr. R. H. Thouless, Professor O. L. Zangwill, Dr. Boris Semeonoff, Professor Alec Rodger, Dr. Isobel Blain and Dr. D. E. Broadbent who was especially helpful in the early stages.

Thanks are also due to the long-suffering librarians and archivists of the Royal Society, the National Institute of Industrial Psychology, Cambridge University Library, St. John's College, Gonville and Caius College, Trinity College (all Cambridge) and lastly the Cambridge Psychological Laboratory. I am indebted to the Royal Society for permission to quote from Myers' "Personal Record" of 1942.

Special thanks are due to Hugh and Denis Bartlett. The use that they kindly granted me of their father's unpublished autobiography and other unpublished articles and papers has proved invaluable. All references to these works and quotations from them appear by their permission.

Lastly, thanks are due to Mrs. Sue Hawkes for being able to read my handwriting and doing some excellent typing.

"As the scene recedes into perspective it becomes clear that the Cambridge laboratory and experimental psychology at Cambridge contribute a full half of the history of British experimental psychology. The efforts, successes and failures of Ward, Rivers, Myers and Bartlett at Cambridge provide a continuity against which the extent of British progress in this line can be measured. McDougall's role at London and at Oxford was important but peripheral. Other persons, laboratories and events enter the history, but they appear as symptoms of what was going on. The whole genesis of the practice of experimental psychology in Britain is not, however, so complicated but that we can sketch the chief events in the order of their occurrence from James Ward on." (E. G. Boring 1950)

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ABSTRACT

This thesis deals with the biographies, the academic work and the influence of James Ward, W.H.R. Rivers, C.S. Myers and Sir Frederic Bartlett. Along with Galton, Sully, Spearman and Burt these four men were among the principle founding fathers of British psychology. Ward, Rivers and Myers were largely responsible for establishing psychology at Cambridge, where, under Bartlett, the subject later flourished.

Part 1 of this thesis argues that these Cambridge pioneers have not yet received the historical attention which befits their cardinal position in British psychology.

Part 2 describes Ward's philosophy, systematic psychology and his advocacy of psychophysics. The importance for Ward's thought of Bain, Lotze and Fechner and more generally, of British Associationism and neo Hegelian Idealism, are described. A biography of Ward is presented with special reference to his long struggle to establish psychophysics at Cambridge between 1877 and 1897.

Part 3 describes the consolidation of psychology under Rivers and Myers between 1897 and 1922. The life of each man is described illustrating their common background in medicine, anthropology and early experimental psychology. Their work on "Shell Shock" in World War I, their work in experimental and cross cultural

psychology, and Myers' massive contribution to industrial psychology, through his N.I.I.P., are outlined.

Part 4 looks at the further growth of Cambridge psychology under Sir Frederic Bartlett from 1922 - 1939. His main contributions, it is argued, were; as an experimentalist; as a psychological theorist; as a promoter of applied psychology; as a respected and influential teacher. Special attention is paid to Remembering.

Part 5 sums up the work of the Cambridge School. As a detailed history the thesis ends with 1939 but this last section also deals briefly with the influence of the Cambridge School since that date and describes the later work of Bartlett.

PART ONE

INTRODUCTION

"Can history be revised? Yes." (E.G. Boring 1950)

CHAPTER 1

THE AIMS OF THIS THESIS AND THE TERM "CAMBRIDGE SCHOOL"

A. The Cambridge School

This thesis attempts to record the contribution made to British psychology by four of its founding fathers. James Ward, W.H.R. Rivers, C.S. Myers and Sir Frederic Bartlett were, between them, largely responsible for the institution, early growth and flowering of experimental psychology at Cambridge. As a detailed study of these four men this thesis covers the years from 1877 to 1939. The starting date marks the first attempt, in Great Britain, to set up a psychological laboratory. This attempt was made by James Ward at Cambridge. The concluding date was chosen because of the rapid developments in psychology provoked by World War II. Interestingly enough Cambridge again was the scene of many of these developments. Notably, these included the growth of applied psychology, psychometrics and aptitude testing and, more profoundly, the use of machine and computer analogies. This area which blossomed after the war owed much to the war time work carried out at Cambridge by a protege of Bartlett's, Kenneth Craik. But these topics deserve a thesis to themselves. There has only been space to hint at the role of Cambridge after 1939 in this work (see Postscript - Part 5).

Inevitably, in dealing with the contribution of these four men a good deal of the history of Cambridge psychology has also been covered. However, this thesis makes no pretence to be a full account of all developments at Cambridge. The main purpose is to record the efforts of the four most

important Cambridge psychologists in the period concerned.

To these four men I have attached the label of the "Cambridge School". My usage is however a little unusual and perhaps deserves a note of explanation. Psychology at Cambridge has never produced a tightly knit body of thought, an all embracing psychological system or even a group of disciples blindly following its edicts. My Cambridge School is not, it must be emphasised, a school in the sense that are, or were, the psychoanalysts, the psychometricians of University College, London, the Skinnerians or even the old hormic theorists. But nevertheless Ward, Rivers, Myers and Bartlett do constitute a school in two senses of the word.

Firstly, as it is intended to demonstrate in this thesis, there is a distinct intellectual thread running through their work. Bartlett, Myers and Rivers were all extremely practical, eclectic psychologists who adopted a largely atheoretical, problem solving approach in preference to adhering to any one school of thought. James Ward owes his membership to the school firstly because he played an important political and organising role in establishing Cambridge psychology. In addition, however, he provided both inspiring ideas such as the activity of memory so important to Bartlett, and also a more general philosophical underpinning.

The second sense of the term Cambridge School is that of a training school for latterly eminent psychologists. Bartlett,especially, was an extraordinarily successful teacher. Before the war pupils of Myers and Rivers such

as Mace, Sprott, Thouless, Banister and Pear[1] became important figures. After the war Bartlett's pupils such as Broadbent, Zangwill, Oldfield, Heim, Rodger, P.E. and M.D. Vernon and Gregory have all made notable contributions to British psychology. To stress the point, in 1957, no less than 10 of the 16 chairs in psychology in the U.K. were held by students trained by Bartlett or Myers. Of course these pupils have not formed a distinct, strictly defined, school. Indeed the Cambridge School was so eclectic that it would have been a form of academic betrayal if they had. However, both from personal testimony and from the work undertaken by these pupils, it is fairly clear that in many cases their training in the Cambridge School was of considerable importance. Thus although the core of the Cambridge School contains only four members it is readily apparent that the transmission of its influence has occurred widely throughout British psychology. Thus the psychology of the Cambridge School has spread far beyond its original four members.

Such is the usage of the term Cambridge School for the purposes of this thesis. It refers mainly to the four core members. However many of its tenets have been taken up by a large number of pupils forming a more diffuse and extended Cambridge School, the nature of which should become clearer later in the thesis.

B. The Importance of the Cambridge School

James Ward, at about the same time as James Sully, translated and imported the work of Weber and Fechner on psychophysics. He was also an ardent opponent of the generally anti scientific idealism argued by Bradley and

1 **Pear was taught by Myers at Kings College, London and not at Cambridge.**

Green in the late 19th century. If he had been allowed Ward would have set up a psychological laboratory in 1877, two years ahead of Wundt and William James. W.H.R. Rivers was Britain's first experimental psychologist, performing pioneering experiments on fatigue and drugs, colour vision and visual illusions. C.S. Myers organised, and paid for, Britain's first purpose built psychological laboratory. Not content with this, in 1921, he built up his National Institute of Industrial Psychology which pioneered nearly the whole field of industrial and occupational psychology in this country. Bartlett's Remembering, of 1932, is almost universally regarded as a masterpiece and a major contribution to the study of memory. Ward, Myers and Rivers started the British Journal of Psychology in 1904. Between them, and with Bartlett, they edited this journal until 1948. Rivers and Myers were important in propagating the use of psychological methods in psychiatric medicine. In addition they more or less started the field of cross cultural psychology.

Even this cursory account reveals that the Cambridge School occupies a large chunk of the early history of British psychology. Indeed it is arguable that Cambridge was in the forefront of psychology throughout the period 1877 - 1939, with its only serious rival being University College, London, which was led, at different times, by James Sully, Charles Spearman and Cyril Burt.

Perhaps the School's most important single contribution was to found a distinctly British or rather, Cambridge, way of doing psychology. As Myers and Bartlett often

wryly remarked there was a veritable plethora of schools within psychology in the first half of this century. There were Gestaltism, Structuralism, Behaviourism, Factor Analysis, Psychoanalysis, the Personalistic approach, Hormic theories and some others too. The Cambridge School sailed merrily through this era. It selected what it wished from each of these approaches (for it was ignorant of none) but never swore allegiance to any single one of them. In doing so it founded a tradition which was eclectic but also practical, down to earth, free from jargon, which showed a respect for careful experimentation and observation and which nevertheless was aware of the dangers of the artificiality in laboratory situations. Such a stance was almost calculated not to draw attention to itself. It was after all so close to common sense.

This indeed may be one of the reasons why this important component of British psychology has achieved so little recognition in the history of psychology. The Cambridge School is notoriously elusive and when one does define it - it sounds disturbingly like common sense.

A second reason is that most of the major histories of psychology have been written in America, by Americans. As we shall see below when British historians have dealt with the Cambridge School, they have not surprisingly, tended to be both lengthier and more enthusiastic than their American counterparts. But this is not just a matter of nationality. The general passing over of the Cambridge School also revolves around the problem of "presentism". To a discussion of this we now turn.

C. "Presentism" and its Implications for the History of Psychology

The problem of "presentism" was defined by Mackenzie and Mackenzie (1974) in their call for "a revised systematic approach to the history of psychology". Behaviourism or at least heavily behaviouristic thinking, has, they argue, until fairly recently, dominated psychology and especially so in America. Thus they claim that in the apparent absence of any viable alternative, behaviourism has frequently been tacitly accepted, by historians, as the only possible form a scientific psychology could take. Thus histories of psychology have, all too often, "been influenced by a tendency to see previous theories as signposts along the road to Behaviourism." They go on to state that the time is now ripe to reassess many parts of the history of psychology in order "to determine the major themes and influences in Psychology's history in a treatment that is scientific but free of any supposed Behaviourist bias."

Of course their case assumes that firstly behaviourism was a dominant force in psychology and secondly that its star is now well and truly waning. I do not intend to argue these assumptions here. Mackenzie (1974, published 1977) has exhaustively recorded the origins, heyday and decline of behaviourism. Koch in many volumes (1959, 1963) has also charted this process in laborious detail. In essence the golden age of behaviourism was between the world wars. Palermo (1970, 1971) argues that during this time the school most closely approached what Kuhn has called a paradigm (Kuhn 1962). Behaviourism was enthusiastically

pursued in America leading Koch to state, "It required close on twenty years of frantic pursuit of such a strategy for the hypothesis to emerge that it was perhaps a bit over optimistic." The greatest disappointments were possibly the lack of practical applications and lack of progress towards unifying concepts. Meanwhile the plotting of the atomistic details of reinforcement schedules and extinction curves continued apace. Rather like Alice and the Red Queen behaviourists seemed to have to run at full pelt to get nowhere in particular. Eventually as Koch puts it, "This disabusement..... at some time in the fifties led to a remarkable liberalisation of behaviourist methodology."

Behaviourism has become increasingly liberalised ever since. The only ardent body of disciples who remain are perhaps the Skinnerians. During the same period psychology has diversified to a remarkable extent. The discipline has rediscovered Piaget, incorporated Chomskyan *psycho*linguistics, heartily embraced information theory and computer analogies and also rediscovered that human beings are conscious, have imagery, cognitive structures, form concepts and so on (see Holt's 1964, "The return of the Ostracised" and Kessel's 1972 "Imagery: a dimension of mind rediscovered").

Perhaps then the only way to view our multifarious discipline is in the way suggested by Beloff, (1973) which is as a collection of "Psychological Sciences". For the philosophical unity of an associationist Wundtian or a determinist Watsonian school is most clearly missing. It would undoubtedly require something of an epistemological gymnast to unite today's discipline, the most notable characteristic of which is probably its heterogeneity.

Mackenzie and Mackenzie's point of view, with which the current author heartily concurs, now becomes clear. Even into the late 1950's behaviourism exercised a considerable hold both on practising psychologists and thereby on the discipline's historians. Today there is no such comparable dominating ethos. Psychology now has room for far more than behaviourism. This fact should enable historians to take a broader view than they have been able to in the past. The recent nature of the liberalisation of psychology is amusingly described by Palermo, a disabused behaviourist, who has worked throughout this era. He writes (1970);

> "Some fifteen years ago.... proposing a symposium on imagery at a psychological convention might have been considered a joke. Most hard nosed experimental psychologists would not even have set aside their copies of Modern learning theory ..... long enough to notice such a symposium!"

However Palermo is not quite correct. If he had suggested such a topic in Britain he would most certainly have not been laughed at - at almost any time in the history of British psychology. For between the wars, in the heyday of behaviourism the Professor of Psychology at the prestigous University of Cambridge, Frederic Bartlett, had a consuming interest in imagery, as did many others. Americans appeared not to notice this until Miller, Gal anter and Pribram reminded them in 1958. Since then, Bartlettian notions such as human use of imagery but also the activity of memory, the strong interconnections of perception, memory and thought and even the notion of schema have become commonplace in the "new" Cognitive Psychology of which Neisser (1967, 1976) is perhaps the leading figure.

As well as confirming the point that many of Bartlett's ideas currently <u>are</u> being reconsidered the above discussion reminds us that the Cambridge School remained strongly anti behaviourist throughout the behaviourist era.[1] Thus if Mackenzie and Mackenzie's thesis is correct then we should find both scant and uncomplimentary coverage of the Cambridge School in the history of psychology.

D. Previous Historical Treatments of the Cambridge School

Ward, Rivers, Myers and Bartlett are not suffering unduly from over exposure in the literature. As we have noted we should expect this for two reasons. Firstly most histories have been American and secondly have therefore tended to be more behaviourist.

Brett's history (1921) and Murphy's (1929) provide an interesting contrast in their treatment of Ward. Brett, writing in Toronto, Canada, devotes ten pages to Ward. Murphy devotes seven lines. This is all the more surprising when he writes, of Britain, that "Leadership in psychology was captured by the school of which James Ward was the leading representative". Brett on the other hand describes, at length, Ward's role in demolishing associationism and stressing the unity of individual experience. To grasp Ward's idea that "life and growth belong to the mind as well as to the body", writes Brett, "is more important than disputing details, for out of the idea comes inspiration". Flugel, in Britain, (1933) also gives Ward his rightful place.

[1] This statement refers only to the years prior to 1939. After the war many of Bartlett's pupils adopted a liberalised form of behaviourism - most notably Broadbent. This issue is returned to in Parts 4 and 5.

Kantor, as late as 1963, cannot, in all justice, be said to have written a Scientific Evolution of Psychology. Rather he wrote an account of the evolution of behaviourism. Also in America, Esper (1964) and even Boring (1950) tend to share the scant regard given by Kantor to Ward, Rivers, Myers and Bartlett. True, Bartlett's experimental achievements in Remembering are usually mentioned. But Ward, who gave Bartlett many of his ideas, is invariably portrayed as the last remnant of a bygone prescientific age. It is interesting to note here that Shotter (1974, 1975) and Joynson (1974) have both argued that Ward's emphasis on the holistic nature of experience is a genuine underpinning for modern 'humanistic' or 'personalistic' psychology. Allport (1950) has also argued for the value of the Leibnitzian tradition, of which Ward was a part, in the psychology of personality. Joynson further points out that Bartlett, in the 1930's, put forward many criticisms of behaviourism which were unfortunately more or less disregarded for some 30 years - to psychology's cost.

Thus previous historical treatments of the Cambridge School seem to be not only scant but also somewhat misleading, especially in light of recent progress in psychology. The only possible exception would seem to be in the 1964 Short History of British Psychology by the British historian Hearnshaw. He has, owing to his subject matter, rather more space for Ward, Rivers, Myers and Bartlett. He tends, however, to play down their positive contribution. Of Ward's psychology he writes (p.139), "On the whole it is a point of view that is unfriendly to scientific progress." Hearnshaw was also unaware of the

considerable revival of Barlettian ideas that was to occur after he was writing. Therefore his account would, one imagines, be a little different and more enthusiastic if he were writing today.

E. The Two Aims of this Thesis

The argument above leads me to state the two main purposes of this work.

The first stems from the fact that much of the history of early Cambridge psychology has, quite simply, never been written. For example the story of Ward, Rivers and Myers' 35 year struggle against the stubborn University authorities to establish psychology has never before been fully recorded. Yet it is a fascinating and illuminating story, explaining to some extent why British psychology started so very slowly. Furthermore not one detailed biography has yet appeared of Ward, Rivers, Myers or Bartlett. This is astonishing when one recalls that the last three were Fellows of the Royal Society while Myers and Bartlett were considerable public figures. I have been greatly aided in this first aim by the access I have been allowed to large amounts of previously unpublished and indeed previously unearthed material (see Acknowledgements).

The second, and the minor, aim of this work is simply to take up the challenge of Mackenzie and Mackenzie. That is to give a, hopefully, unbiased account of Cambridge psychology which historically has always provided an alternative to behaviourism. In the relatively eclectic climate of psychology today, alternatives to behaviourism

are well in evidence and there, is, therefore, reason to suppose that this second aim is not a forlorn one.

CHAPTER 2

THE BACKGROUND TO THE ORIGINS OF CAMBRIDGE PSYCHOLOGY

A. Introduction

It is the intention in this chapter to provide a brief overview of the social and intellectual background which gave birth to Cambridge psychology. It is also hoped to explain, to some extent at least, why it was that experimental psychology started so slowly in Britain while in Germany it flourished.

In the first section, below, an account is given of the larger cultural, social and religious "zeitgeist" of the late 19th century in Britain. On the whole, as we shall see, public opinion was unfavourable to psychology for reasons that were, in part at least, religious. These objections, in turn, were part of a larger religious reaction to science in general which had been fired not only by the recent controversy over evolution, but also by the importation of German Idealism. On the other hand, as the next section shows, the British philosophical background was extremely favourable towards the growth of psychology. Associationism had culminated in the work of Bain who held that mental events were lawful and could thus be profitably studied. Furthermore he had a large place for physiology in his system.

The last section in this chapter examines the "Irony of Leipzig". It compares the Germany of the 1860's and 1870's with Britain at the same time. Thus it attempts to explain why the home of Idealism developed an Associationist psychology while, after a slow start, the home of

Associationism developed a psychology heavily infused with Idealism.

B. The Religious, Philosophical and Cultural Climate; the "Revolution of the Dons"

From 1877 until 1912 the struggle to establish Cambridge psychology is the story of a "small group of politically motivated men" - to borrow a famous phrase. The precise machinations of this battle and the reactions at the various points of decision such as the Senate and the various Boards of Studies are outlined in Chapter 4.

The immediate backcloth to these events was a significant social, intellectual and political upheaval taking place within the University. Rothblat (1968) has called this process the "Revolution of the Dons". The process is also covered in much detail by historians such as Winstanley (1947).

At Cambridge one of psychology's allies, Henry Sidgwick, was a leading figure in this "Revolution". He was a powerful political force within the University, as the short sketch of his career in Chapter 4 indicates. The main aims of the liberal dons of the 1860's, 70's and 80's were:-

a. To expand the availability of the University's courses - especially to women students.

b. To encourage the University to expand and adapt, especially to encompass the fast developing natural sciences.

c. To abolish the notorious "Religious Tests".

> These tests effectively prevented any person of the slightest religious nonconformity from taking up any position of power, within any of the University's colleges. The tests were finally repealed by Act of Parliament in 1871, but conformist religious influences maintained their stranglehold on the University for many years to come. (See Winstanley 1947 pp 36 - 90.)

Not surprisingly these liberal dons met with fierce opposition within the University. But this struggle was merely a microcosm of a debate being conducted on the outside on a far grander scale. Part of this debate is well described by Hearnshaw (1964 p 120) under the heading of "Scientific Materialism". He writes;

> "In 1870 Great Britain was still in the van of psychological progress. Bain, Darwin, Spencer, Galton, Maudsley and Carpenter were all alive and near the height of their powers. The laboratories of Germany and America had not yet been established, and, although German work in the physiology and psychology of the senses was already impressive, it was specialised and restricted in scope. To psychology generally Ribot could not unjustifiably claim that Britain had contributed most. And indeed the prospects for the growth of a biologically orientated psychology, founded on the theory of evolution and the physiology of the nervous system, and assimilating all that was of value in the native associationist tradition, seemed not unpromising. It was not to be. British psychology had been for too long too closely identified with a particular philosophical tradition, anti-religious in general tone, and this tradition was about to suffer an eclipse. No science is so self-sufficient as to be wholly immune to changes in philosophic climate. Nineteenth-century psychology, barely emerged from infancy, was especially susceptible to

> them. When in the third quarter of the nineteenth century British philosophers, renouncing their native philosophic tradition, flocked to the idealistic banner, the development of psychology was necessarily affected. In German and American universities a rapid growth of psychological departments took place in the last two decades of the century. British universities antagonistically held aloof, and for at least two generations the academic development of psychology was retarded."

In essence then philosophers, such as T.H. Green and Bradley, both at Oxford, were taking up, in Britain, the Idealist cause which had, so recently, fallen out of favour in Germany. This seems to have happened for two reasons. Firstly, the joint attacks on religion by the long-standing British materialistic tradition and the rise of natural science had eventually provoked a reaction in religious circles. Secondly Kant's Critique and Hegel's Logic had finally appeared in translation in 1838 and 1874 respectively, (the first taking 57 years and the second 58 years). The effect of this, as Hearnshaw puts it, was that, "The neo-Hegelian form of idealism was much stronger in Great Britain than elsewhere; it tuned in with and lent support to the evangelical conscience which was so strong in Victorian England, and which scientific materialism seemed to threaten."

It was a strange era, with scientists such as Tyndall telling the British Association, in his Presidential address, of the omnipotence of science. Meanwhile T.H. Huxley apparently eager to pre-empt John Watson was stating that consciousness was a mere epiphenomenon and that psychology was about the functions of the brain and nervous system.

Thus both in the "zeitgeist", as Boring (1950) and Hearnshaw (1969) put it, as well as in the philosophy of T.H. Green and Bradley (to whose argument with Ward we turn in Chapter 5) there existed forces distinctly opposed to the encroachment of science. Psychology was perhaps seen as the biggest threat of all, as it (heresy of heresies) attempted to scientifically dissect the soul! Hence it came in for perhaps more than its fair share of criticism from theologians such as Martineau, W.G. Ward and J.H. Newman.

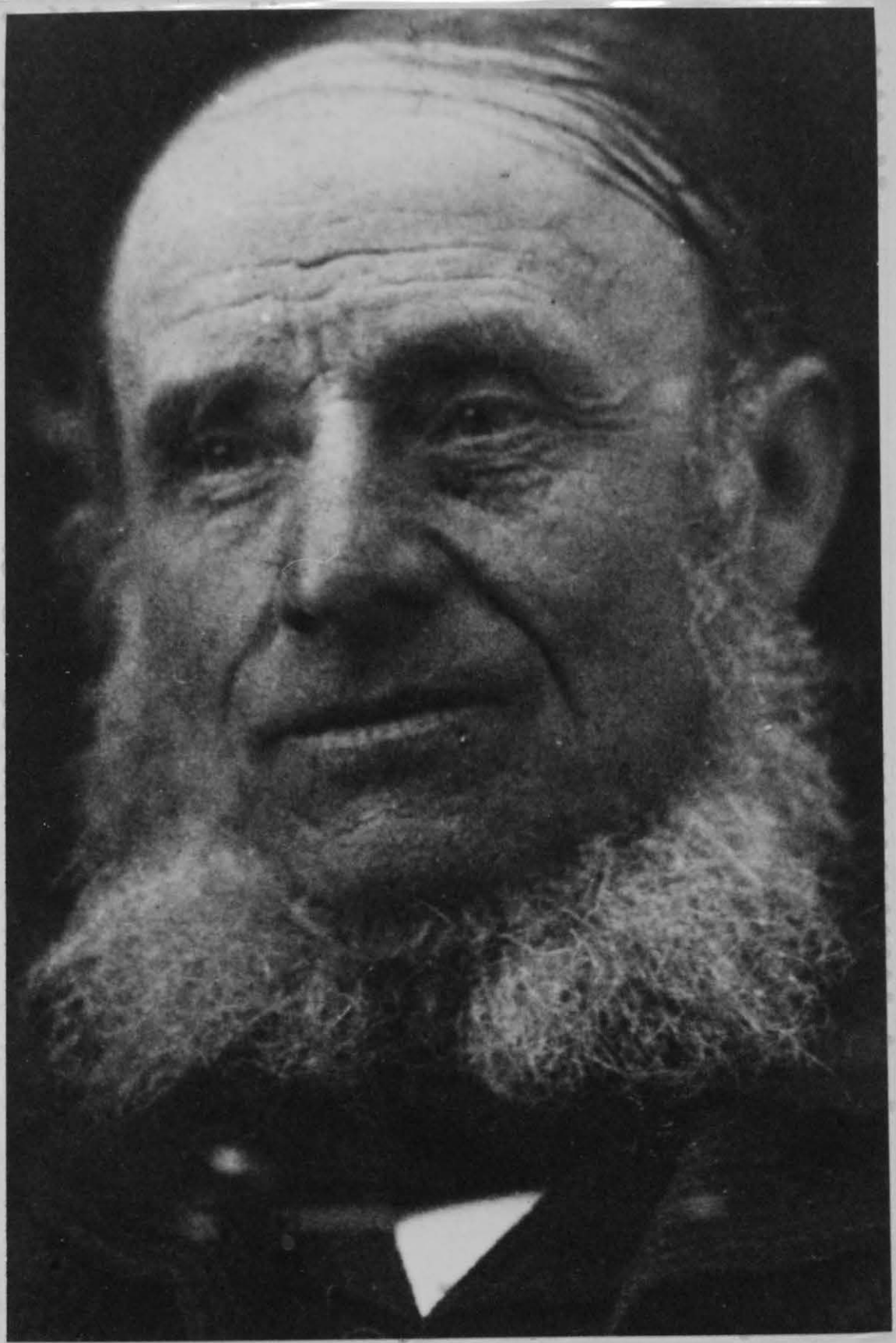
Thus the liberal dons of Cambridge who wished to further scientific progress, were faced by both popular and academic opposition. The conflict reached such proportions that in 1869 the Metaphysical Society was established to discuss the head-on conflict of religion and science. The society comprised Archbishops, Bishops, Catholic Cardinals, natural scientists, philosophers and psychologists (including Sidgwick, Croom Robertson and Sully).

In 1877, when James Ward tried to found the country's first psychological laboratory, Cambridge University was not the most enlightened or libertarian institution in the world. Its habitual conservatism however, was not any lessened by this outside cultural movement. In fact, in Chapter 4, we shall see that the continuing opposition to psychology throughout the 1880's and 90's was largely religiously based. This view is largely that propounded by Hearnshaw in 1969. He writes, "the conservatism of British universities must take a large share of the blame,

PLATE 1

Alexander Bain 1818 - 1903

"The culmination of association" and "a cardinal point in the historical orientation of psychology". E. G. Boring.



though of course it could be argued that the universities merely reflected the national Zeitgeist."

It is ironic indeed that this idealist/religious reaction should play a part in delaying the development of British psychology. For to some extent the British Associationist tradition had much to offer not only to Wundt's sensationism but also to Pavlov's reflexology and Watson's behaviourism. But we return to this argument later.

C. The Intellectual Background; British Associationism and Alexander Bain

The tradition of British Associationism is generally regarded as starting with John Locke, in the fourth edition of his Essay Concerning Human Understanding in 1700 (cf Flugel 1933, Warren 1921, Boring 1950). However Locke's rivals for this honour include Aristotle and Hobbes (Human Nature 1684, Leviathan 1651). In the 150 years following Locke such prominent philosophers as Hume, Berkeley, Hartley, the two Mills and lastly Bain worked away within the tradition. The school had two fundamental assumptions. The first was the famous "tabula rasa" concept of the mind proposed by Locke, on which sensory input was the enscribing stylus. The second followed from this and stated that all experience arises in a lawful manner from primitive raw ideas and sense data.

Warren (op.cit.) summarises the school's work under three headings;

1. The formulation of the laws by which association proceeds. These were variations on the three

basic principles of contrast, contiguity and similarity.

2. The attempt to analyse all sorts of mental phenomena by reducing them to the results of these associative processes.

3. The application of these results to other fields such as ethics, sociology, aesthetics and epistemology.

Locke, Hobbes, Berkeley and Hume are often referred to as British Empiricists as they were mostly concerned with the epistemological status of the doctrine. Only with James Mill's Analysis of the Human Mind (1829) does the school become more psychological in character. Warren writes (op.cit.);

> "James Mill viewed the human mind as a species of machine. It is set going by outside forces (sensory stimuli) and proceeds to operate as a physical mechanism. The machinery of the mind is the associative process, and the elder Mill conceives of this acting like a physical force, mechanically."

John Stuart Mill, who was a close friend of Bain, continued his father's work. He differed however in one important respect, namely that mere introspection could not break up complex whole experiences into simple constituent elements. Some sort of "transformation" occurred in complex experiences analogous to an irreversible chemical reaction. Such a view, of course, is something of a forerunner of the ideas of the Gestaltists.

The school culminated in the work of Alexander Bain. He was one of the first thinkers, certainly in Britain and probably in the world, to devote the majority of his academic effort to what we now understand as psychology. His first book The Senses and the Intellect came in 1855 and his second The Emotions and the Will in 1859. These books, which formed the two volumes of one work, became, "the standard British psychological text for almost a half a century, until Stout's replaced it" (Boring 1950 p 235).

Flugel writes of Bain that, "He owes his place in history to his power of laborious collation and clarification of data and systematic exposition of results." In fact Bain seized upon diverse developments in physiology, mental philosophy, neurology and mental illness and attempted to weave them into a coherent discipline. His work is essentially an attempt to combine British Associationism with contemporaneous work in these related fields. Thus, as Boring writes, "He represented the culmination of associationism and the beginnings of its absorption into physiological psychology."

It is useful to discuss Bain's work in its four parts. In The Senses we have a minutely considered account of the structure and functions of the brain, spinal cord, the motor system and the organs of the internal and external senses. The purely anatomical and the purely psychological aspects of these organs lie side by side. Such an account had been attempted years before by Herbart. However Bain had the advantage of considerable advances in physiology. To bring home the innovatory nature of Bain's

work to the modern reader (who now finds the close links of body and mind to be self evident) a passage of page 10 is salutary. Here Bain presents, "Proofs that the Brain is the principal organ of the mind." These "proofs" consist of (alleged) correlations of brain size and intelligence, localised pain (headaches) and the effects of brain injury. Yet even these, now obvious, statements were to make Bain the target of the considerable forces of the religious/idealist reaction. For in them they scented materialism. Bain's main physiological sources were Quain's Elements of Descriptive and Practical Anatomy (1828) and Carpenter's Principles of Human Physiology (1842). He also quotes from Bell, Todd and Bowman. He also used the German work of Müller (translated 1842) and Weber (eg pp 181, 228) but not to a large extent.

Bain's treatment of The Intellect is so much orthodox Associationism that it requires little more explanation. Even his treatment of creativity, in his section on "Constructive Associationism", fails to break the mechanical shackles Bain would impose on the human mind. He writes (p 572), "I mean to afford that the intellectual forces operating in those creating are no other than the associating forces already discussed. For the new combinations grow out of elements already in the possession of the mind and are brought forward according to the laws above laid down."

He returns to physiology in The Emotions. The physical and mental again lie side by side. In later editions Darwinism and the importance of instincts are incorporated. Somewhat in anticipation of the James-Lange

theory of emotion is Bain's idea that sensations produced both emotions and nervous correlates which produced an impulse towards motor movement. He proposed an inevitable correlation between the size of the affective component and the strength of the motor impulse attached to any sensation. Interestingly enough Bain also proposed (p 25) that introspection was not sufficient to study emotion. A "natural history" or observational approach as well as quantitative methods should be used, he claimed.

Bain is notoriously obscure over the issue of The Will. He makes many contradictory statements on the topic of free will and one only has to read Bain's discussion on "Liberty and Necessity" and compare it with Hearnshaw's opinions (p 12) to realise the depth of the problem. Indeed Flugel (p 82) writes, "It is generally considered, however, that Bain hedged on the problem of the Will."

In general however, it is probably fair to say that Bain held to a view of psychophysical parallelism. That is, both mind and body were closed, lawful systems functioning in parallel. The former operated by the laws of association together with certain innate and instinctive tendencies. The latter obeyed the laws of physics, chemistry and physiology. Bain tended, however, to stress the material side, writing in The Senses, "The mind is completely at the mercy of the bodily conditions." Then, as Flugel suggests, probably to stave off religious objections, Bain developed a notion of spontaneous activity. This notion was very physiological in character, depending on innate motor patterns and a "residual" level of nervous

and cerebral activity. Thus Bain clearly veers towards epiphenomenalism with its inherent implications of passivity, determinism and materialism.

It was these implications which James Ward was to find so unacceptable. Bain's view precluded any notion of an "active self". Brett (p 643) points out that Bain valued Ward's theories, "as a sign of progress enfeebled by the tendency to go too far." The "active self" was a dangerous return to obscurantism and a "nucleus and hiding place for mysticism" (op.cit. p 645). Meanwhile Ward was equally sure that, what he saw as the self evident, unitary, active and free nature of everyday experience should provide the cornerstone of any attempt to formulate psychology. Thus, as we shall see, Boring is absolutely correct when he writes of Bain, "He is important mostly as a cardinal point in the historical orientation of psychology" (p 236). For he marks the parting of the ways for British and German psychologists. In fusing physiology and associationism he provided the foundations for much of Wundt's work. In proposing, or at least in seeming to propose, a rather passive deterministic model of conduct he provoked James Ward into formulating his alternative of the "active self".

Nevertheless Ward was never slow to draw upon the accumulated wisdom of British Associationism. This work thus plays a <u>positive</u> as well as a <u>negative</u> role in the development of Ward's thinking. Ward did however put the school firmly in its place by highlighting its limitations.

Returning to Bain it should be remembered that he was important in stressing the close connections of psychology and physiology. This was especially the case in his treatment of the senses. In 1893 Michael Foster's sponsorship of studies in the physiology of the senses was to give psychology a new lease of life. But it was Bain who was the first great publicist of these unpopular theories.

Thus ends this account of the British intellectual background which gave birth after a long and protracted labour to psychology. As we shall see continental influences were also to be vital in the birth of British psychology. Fechner, Lotze and Brentano were especially important. But these thinkers were not part of the British zeitgeist. Their importation to Cambridge occurred so much through the work of James Ward that their influence is dealt with in Chapter 5.

D. A Comparison with Germany; The "Irony of Leipzig"

In the person of Alexander Bain there would seem to be sufficient foundation for an experimental psychology in Britain. For his work shows that British physiology of the 1850's and onwards was fairly well advanced. The long standing Associationist tradition was also more than encouraging. It had at times been thoroughly materialistic, but had always held that mental life was lawful.

But as we know experimental psychology started at Leipzig. In Germany the subject mushroomed while in Britain it languished. Yet Britain had been the home of Associationism for over 200 years and it is this tradition,

as Beloff (1973) has remarked, that permeates much of modern psychology. Mackenzie (1974 pp 174 - 5) puts it even more strongly. He writes:

> "It might be suggested that the development in functionalist comparative psychology of a sensationalistic, passive organism model of mental life could follow in part from the continuing influence of the British empiricist tradition, in which the passive organism model had been gradually developed and fully adumbrated from Locke through the Mills. Certainly the continuing influence of the British empiricist and associationist model was invoked many years later by Hull (1943) as providing the thematic background for the psychology of his own day."

Later Mackenzie, referring to early German Psychology writes that, "introspective experimental psychology.... was itself largely a German formalization of the tenets of British Empiricism."

Yet Germany, as we have noted, was the traditional home of an idealist doctrine which held that any psychology was pointless if not impossible. So why then did this curious process occur?

Ben David and Collins (1966) have made an intensive study of this, the "Irony of Leipzig". They arrived at a sociological explanation which depends on the concept of "role hybridisation" which is illustrated in Table 1. (p.28) They claim to have developed an objective "social factors" explanation of the origins of psychology. Role hybridisation for example could only occur when competition in philosophy was less than in physiology. This they claim occurred nowhere else except Germany and therefore all psychology spread from there.

Ross (1967) has criticised this viewpoint claiming that, in the case of America, William James should count as a "self-starter". This is because, she claims, Ben David and Collins define role hybridisation so narrowly as to render it meaningless. For the case of William James, she argues, clearly shows that role hybridisation occurred in more places and in more ways than they imagine. For he too tried to build up a psychology which should, Ross argues, be regarded as an indigenous development and not a slavish importation. Ross goes on to argue that the birth of psychology was due to a subtle interplay of social and intellectual forces.

The case of British psychology would seem to support Ross's argument.

For James Ward provides even more of a clear cut case. He went through almost exactly the reverse process to that of Wundt. He started in philosophy, but found no jobs there, went to physiology and thence tried to start up psychology. Micro-social factors were clearly at work. Just as Ward found competition in philosophy very high Wundt had found competition in physiology too difficult. But both men, intellectually, finished in a similar position - envisaging the need for the implementation of scientific methods in the traditional problems of mental philosophy. Wundt, as we know, was readily supported. Ward fell foul of the idealist/religious reaction we have discussed above. Cambridge University was sensitive to this reaction and Ward was denied.

Such, essentially is the explanation of the "Irony of Leipzig". Ben David and Collins' explanation is plainly inadequate. For the development of psychology in these early days was indubitably related to large scale social, cultural and religious movements in addition to the rather curious philosophical events of the late 19th century.

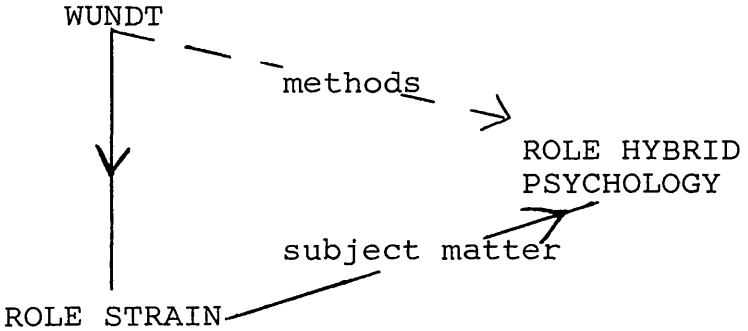
It took Ward, and latterly Rivers and Myers, 35 years to persuade Cambridge to build them a laboratory. They were all men of undoubted intellect and conviction but even they could do little against this predominant "zeitgeist". But to their long and interesting struggle we now turn.

TABLE 1

ROLE HYBRIDISATION

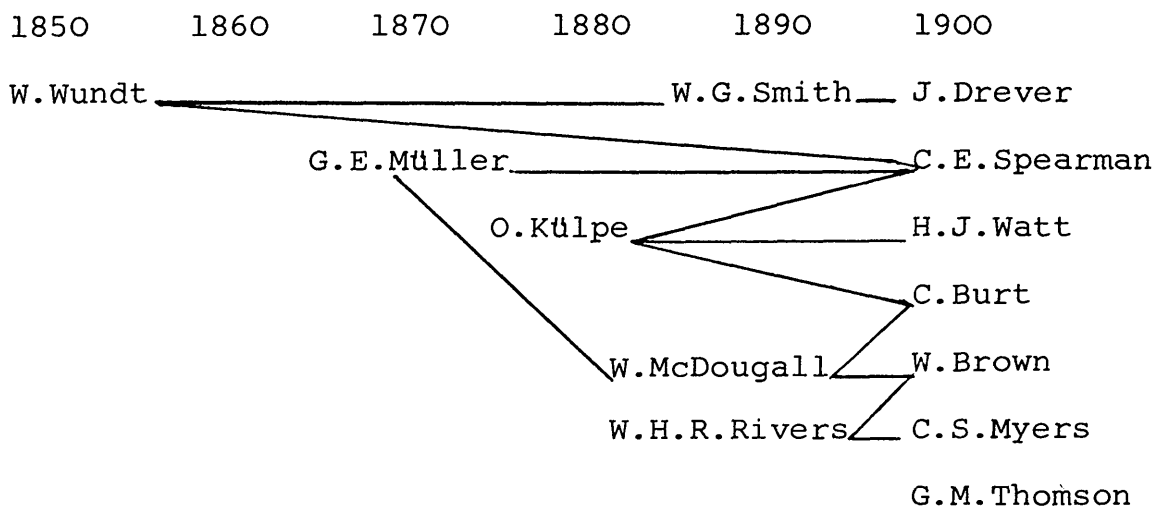
PHYSIOLOGY
(High status in 19th century Germany and thus fierce competition)

PHILOSOPHY
(Lower status and thus fairly low competition)



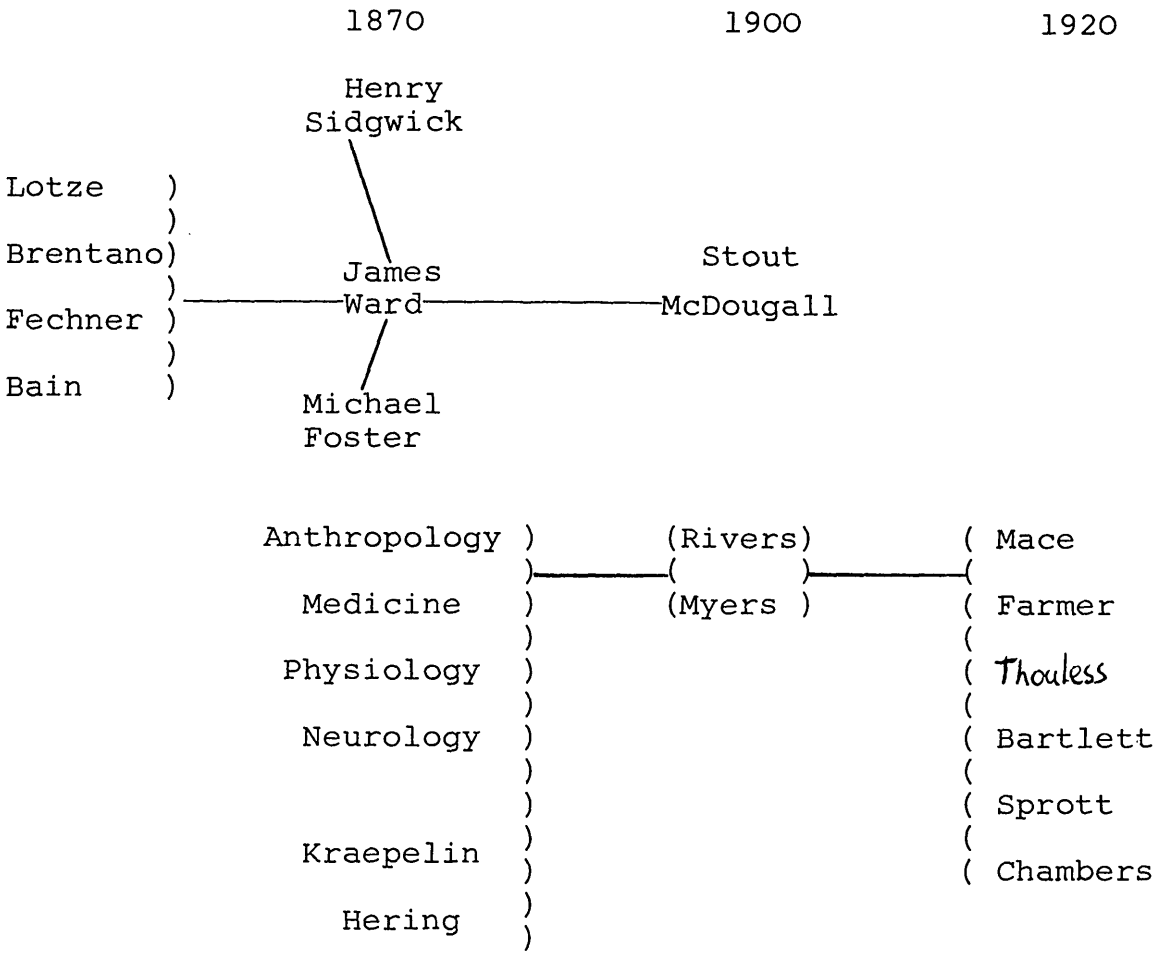
This diagram shows Ben-David and Collins' concept of role-hybridisation. It purports to show how Wundt, a capable physiologist found his professional progress to be slow, owing to high competition. Thus he migrates to a Professorship in Philosophy. In this position, being a trained scientist and having left behind his important peers and reference groups he experiences "role strain". To ease this he creates a "role hybrid" of the methods of physiology and the subject matter of philosophy - psychology.

TABLE 2



The genealogy of early British Psychology as presented by Ben-David and Collins. No mention is made of James Ward, Sully or Stout. Rivers and Thomson the only "self starters" are dismissed as unimportant. Spearman and Burt are portrayed as pupils of German founders. In fact Spearman complained of his unsympathetic treatment in Germany. Burt's work owes : Fairly little : to Külpe's influence. Myers owes even less to McDougall (who was a contemporary of Myers not his teacher) and Müller. On the other hand Spearman and Burt owe a huge amount to Francis Galton while Rivers and Myers were influenced by Ward and these three between them created Britain's first experimental psychology laboratory.

TABLE 3



An alternative and somewhat selective genealogical chart of early Cambridge psychology. This chart only takes account of the main intellectual influences involved. It aims to show that even though British psychology was subject to German influences it very soon took on its own character especially in the applied field - to which area many of Rivers' and Myers' pupils turned.

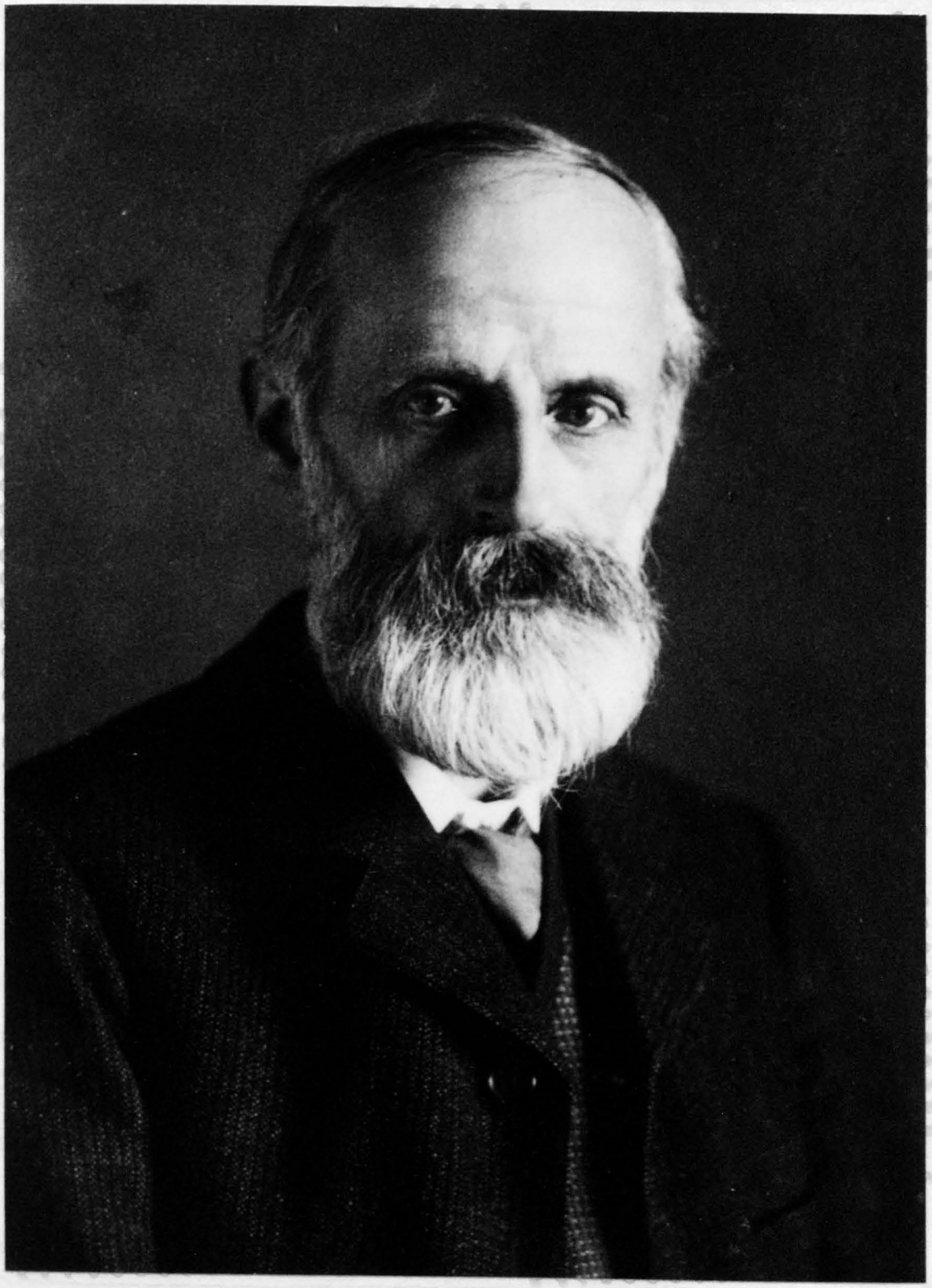
PART TWO

JAMES WARD AND THE ORIGINS OF CAMBRIDGE PSYCHOLOGY

PLATE 2

James Ward 1843 - 1925

"He was, suitably with his psychological outlook, crammed full of activity. In the laboratory he prowled about, ill at ease, more than a bit disgusted". F. C. Bartlett.



CHAPTER 3

JAMES WARD 1843 - 1925; A BIOGRAPHY

(All quotations in this chapter, unless otherwise attributed, are from a memoir of her father by Olwen Ward Campbell - published in Ward 1927)

A. Boyhood; 1843 - 1858

Ward's boyhood is an interesting one. The first thirty or so years of his life were spent in some considerable turmoil. Problems of health, finance, reputation, security and, perhaps most importantly, religion literally plagued the first half of his eighty two years. It was out of this anguish (and from all accounts the word is not an exaggeration) that came his academic viewpoint. Questions such as the existence of God, the nature of knowledge, the form of reality and the nature of man and his mental life were with Ward, in an unusually personal sense, throughout his life.

James Ward was born, the first of eight children, on January 27th 1843 in Hull. He was the son of, shall we say, a somewhat unusual father - whose influence is plain to see in the life of his first born. For James Ward Senior was a fanatical Calvinist, a would be inventor (whose inventions never quite seemed to work) and an inveterate, self-made, multiple bankrupt.

Ward Senior was, however, well educated. Though even this was unusually achieved, through the attentions of his grandfather and not of his father who, we are told, was, "idle and self indulgent". Be that as it may James Ward Senior was, "sincerely devoted to his family and had every

intention of giving them, boys and girls alike, a sound education". He seems to have succeeded in this aim. For, apart from James Ward himself, Arthur (the only other son and separated from James by the birth of six consecutive daughters) later became a Wrangler at Cambridge and Professor at Canning College, Lucknow.

By the time James Ward was eleven his father transferred the family to Liverpool. This was a good period. His father owned a business at Bold Street, Liverpool as well as a country house at Crosby.. After a short time spent locally as a day-boy at the Liverpool Institute, there was enough money to despatch James, as a boarder, to Mostyn House Parkgate - a preparatory school for Rugby. It couldn't last. In June 1856, James, at thirteen and a half, was summoned home to the scene of his father's first business disaster. As if the ignominy were not enough the circumstances were described as, "possibly not the most excusable". James had to return home through lack of money and, in fact, had to spend the next two years of his life without either schooling or occupation to fill his time.

B. Early Theology

This period ended when Ward took his first job, as an apprentice in a Liverpool architect's office. He stayed for four years. He entered the job aged 15, raw, sensitive and immature. He left as a strongly religious, even evangelical, entrant for Spring Hill Theological College - his heart set on the ministry.

He took the architect's job on the basis of drawings he had done for his father's latest invention - some sort of pump. He was unhappy and unfulfilled. Helped by some friends, he started a debating society. It had become commonplace by this time for Ward to discuss philosophical and religious matters with his father (who had just published his only book "God, Man and the Bible"). He apparently found it very easy to transfer these discussions of politics, religion and philosophy to his new society. So much so, in fact, that Ward was able to recall in 1914 (see Bartlett 1925 (1) and Campbell) that everyone told him he was "cut out to be a lawyer or a parson".

But soon, under the influence of his father and a, "rather pious young Wesleyan" acquaintance, religion began to monopolise his life. "Ward was not of a calibre to do anything by halves". He became infected by a religious zeal, teaching at Sunday School, preaching, attending church and studying so much that free time became very rare. This period and the six years to follow at Spring Hill were dogged by lack of money, ill health and almost fanatical hardwork. Campbell explains this by suggesting that Ward's narrow Calvinist upbringing led him to work even harder at theology, partly as an attempt to assuage his guilt feelings. These feelings stemmed from a real doubt about his faith - an almost unthinkable occurrence and one which, she suggests, he attempted to ban from his conscious life and compensate for with fanatical endeavour and zeal.

C. Spring Hill - Training for the Ministry

Months of "severe study" followed Ward's departure from the architect's office in January 1863. In September he began at Spring Hill. His parents were delighted and saved what they could to aid their son. Ward had initial difficulties partly due to his unusual manner but also because of extreme penury. However he was soon recognised for his brilliance and began to express irritation at being held back to everyone else's pace. In the summer of 1864 Ward's father crashed again. This time although his business, now a china shop, was profitable enough, he had squandered money on his latest "scheme".

This particular ill-fated invention was a diving suit. Teething troubles always required just a little more money to get it just right and inevitably Ward Senior was declared bankrupt once more. Something of his character is revealed, however, in the fact that he bobbed up in the cotton trade as soon as Christmas 1865 and made a reasonable living there - for a while.

Perilous though his father's fortunes were, James was supported just well enough to be able to carry on. He began to augment his studies. He passed the London B.A. with honours in the Spring of 1866, having failed at the first attempt due to persistent ill health. In the Autumn of 1866 he started on the London B.Sc. By 1868 he had passed the first part of this as well. With his degree and most of his training behind him, he began to broaden his horizons. Fatefully for British thought he applied for, and won, the "Dr. Williams Scholarship", in the Summer of 1868. This enabled him to spend some considerable

time abroad. But in fact he did not take up the opportunity until the next year - when he had finally finished at Spring Hill.

A friend at the College, who had recently returned from Germany, had developed an "infectious enthusiasm" for Continental philosophy in general and Hegel in particular. Thus it was to Germany that Ward decided to go. The choice was to prove just as fateful for Ward as it was for British psychology.

D. Germany and Lotze

In the Autumn of 1869 Ward, then 26 years old, travelled to Berlin. He enrolled in the Dom Candidaten-Stift, a college for trainee German ministers. Ostensibly, under the terms of his scholarship, he was supposed to further his theological studies. In fact, all he learned in Germany led inexorably to his departure from theological circles.

By all accounts Ward reacted well to his new environment. He found the lectures from Donner and Trendelenburg and the philosophical discussions with his colleagues a great stimulus. Plainly he also felt relieved, at last, to be in some degree of detachment from his family. To his friend at Spring Hill, Wolstenholme, he wrote, "I feel it is absolutely for me, to study for some time in quiet, and free from all doctrinal restraints, and the ministry is incompatible with this". He described himself as, "Enamoured with philosophy".

Still nursing his doubts about his faith, Ward felt obliged to turn down an offer to become the pastor at the

"American Chapel in Berlin". He elected instead to visit Gottingen to attend the lectures of Rudolph Hermann Lotze who had inherited Herbart's chair in Philosophy in 1844. Lotze introduced Ward to the German philosophical tradition and German physiological psychology. It was here that Ward first became fully aware of the work on the one hand of Leibnitz and Kant and on the other of Herbart, Fechner and Helmoltz.

E. From Minister to Academic 1870 - 1873

Ward was back in England in August 1870. The tide had begun to turn. He sought advice from Spring Hill. In a submission, which he hoped would produce advice from his teachers regarding his future career, he pointed out that his aims were now two-fold. The cause of natural science now figured alongside his Christianity.

His major difficulty seems to have been his insistence that religion be treated in what he saw as a proper intellectual fashion. He saw the problems of Christianity as indissolubly linked to philosophy. Thus they should be discussed in an academic fashion with as few preconceptions and doctrinal restraints as possible. He found it hard to swallow that, after his six years of training, a church could not be found which was broad enough to encompass his beliefs. In letters to Wolstenholme he wrote, "How often I have wished I had never known Spring Hill, and how earnestly I do wish it now".

Nevertheless, in October 1870, he was offered a trial period as a pastor at Cambridge. The Congregational church where he took up this appointment was, prophetically,

in Downing Street - the future sight of the Psychological Laboratory. However he only accepted the post with an immense trepidation. As Bartlett puts it, "He accepted with much hesitation, feeling that he no longer held orthodox religious views, and he refused to be ordained" (Bartlett 1925 (1)).

Inevitably, the fifteen months for which he held the post from January 1871 were marked with controversy. Factions of the congregation objected to both the substance and the style of his pastorate. On the other hand many were notably enthusiastic. Ward took the criticism he received very hard. But his realisation that he was not intended for this life grew and is reflected in his letters.

In March 1872 he resigned. He had never been ordained. He joined the University immediately, tided over financially by a Mr. Bond, a leading member of his congregation. He began as a non-collegiate student in the Autumn of 1872 and thence, "working as hard as I can at Philosophy and at Theology", began his illustrious academic career.

"It was done - but it was a shattering business, and it left its mark for ever afterwards", writes Campbell. The shock of throwing up the aims of his life, at 29 years of age, and apparently admitting that he had wasted his last eight years was naturally great. He wrote, "I did not know how utterly I had been crushed - I cannot tell uou how I feel, how one great ruthless heel of fate seems to have stunned and flattened me and my prospects."

But things soon began to improve. In the s pring of 1873 he entered for, and won, a competitive scholarship

in Moral Sciences at Trinity College. Thus began his second career. He was thirty and his connection with Trinity was to last until his death 52 years later. But, before we consider his academic career, let us first ponder over the legacy that was left with Ward by his involvement with religion.

It must be said at the start that the influence of religion in Ward's later thought is not easy to fathom. What is clear, is that he always remained interested in the religious questions involved with philosophy and psychology. Indeed, as we shall see later, he could hardly escape the great debate of the late 19th century posed by the growth of science and the onslaught, as many saw it, of materialism. On the personal side Ward retained his own rather individual faith. He remained, wrote his daughter, at the very least a "Christian theist".

The nature of his religious thinking is best indicated in his two sets of Gifford Lectures of 1899 and 1911. In these is also perhaps the clue of the way in which religion permeated his thinking, but we return to these issues in Chapter 5.

F. Early Academic Career 1872 - 1886

Ward started his academic career with a predominantly theological and philosophical background. However his trips to Germany, as well as imbuing him with a generally Lotzean viewpoint, had also served to attract him to physiology and psychophysics. In fact, the early part of his life at Trinity was spent in physiological studies. But eventually psychology claimed him. How then did Ward become a psychologist?

In 1873, after obtaining his scholarship, he settled into the grind required for the second half of his London B.Sc. In the Autumn to everyone's astonishment he failed this miserably. The following year he redeemed himself by being placed alone in the first class of the Moral Sciences Tripos[1]. In this same year, 1874, he also became a London M.A. winning a gold medal in Philosophy.

On the basis of his academic successes, in 1875 Ward decided to present a fellowship dissertation to Trinity College. Entitled "The Relation of Physiology to Psychology: an Essay", most of the work appears in Mind 1876 entitled "An Attempt to Interpret Fechner's Law". The paper does exactly that. In it Ward examines the logical consequences for physiology, philosophy and psychology of Fechner's famous work. Not surprisingly his dissertation threw his electors into considerable disarray. For Fechner's experiments had only once before been presented in English - in James Sully's Sensation and Intuition of 1874. Also references to Herbart, Volkmann, Brentano and Wundt are scattered liberally throughout the paper. As we have seen (in Chapter 2) even far better known work, such as that of Hegel and Kant was taking upwards of 50 years to obtain recognition in Britain. Nevertheless Ward obtained election to a fellowship - Trinity's first in Moral Science.

1 The respectability of the Moral Sciences Tripos had just been restored after a campaign, led by Sidgwick, had persuaded the Senate to support the Moral Sciences more enthusiastically. In the 1860's the M.S.T. had been of "particularly evil repute" but in the 1870's the Tripos attracted good candidates and demanded high standards. (For more details see Winstanley 1947 pp 186 - 9)

The hand of Henry Sidgwick hangs heavy over Ward's career from this point. Earlier he had campaigned for the abolition of the religious tests - which Ward would certainly not have felt able to pass. Thus he would not have become eligible for a fellowship. More recently Sidgwick had been the most active of what Winstanley (1947) has called the "Trinity Reformers" in establishing the respectability of the Moral Sciences - and hence their suitability for Fellowships.

From this point on these two men became firm friends and allies. Both had resigned posts for religious reasons, both were extreme liberals, both favoured female emancipation and both saw a need for scientific procedures within mental philosophy, Sidgwick for psychical research and Ward for psychophysics.

But here, in the Summer of 1876, Ward's career once more reached a crisis. There were not any jobs for moral science tutors. So Ward takes himself once more to Germany - and this time to Leipzig.

In Leipzig Wundt was preparing to set up a demonstration laboratory, just as James was in America. Ward worked with Ludwig in his physiological laboratory. His work at physiology was, apparently, very successful.

In May 1877 he returned to Cambridge. Here he found some tutoring - or rather Sidgwick found some tutoring for him. It was Sidgwick too who promoted Ward's (unsuccessful) application to the Chair in Philosophy at Aberdeen in 1880. It was in fact after 2½ years of specially arranged lecturing in psychology, at Trinity, that in April 1881

Ward was officially appointed as a lecturer. This again was, "by the good offices of Sidgwick". He was immensely and understandably relieved, at the age of 38, to have obtained his first secure job in his chosen academic sphere.

But the preceding years had not been wasted. In 1874 he had published two papers in Nature entitled "Animal Locomotion". In 1875 there followed his fellowship dissertation. In 1879, after a third trip to Germany he had begun work in [1]Foster's laboratory. His work with Foster culminated with the publication of "Some notes on the physiology of the nervous system of the Crayfish" (Ward 1879 (2)). Foster was so impressed as to lament the loss of Ward to physiology calling him "a physiologist spoiled". He also saw fit to present a summary of the paper (Ward 1879 (1)) to the Royal Society. In the same year Ward published "The Vitality of the Common Snail" and wrote an article on Herbart for the Encyclopedia Britannica. Also in these years, prior to his official appointment, his psychological system was beginning to take shape. Early papers on Ward's psychology were read to the Moral Sciences Club. Some of them, dating from 1875, are still available in the Psychological Laboratory archives. Some of these papers were later published in various journals (see Ward 1883 (1,2); 1887 (1)). Also, in 1877, Ward first applied for psychophysical apparatus - but this long and complex story is told in the following chapter. But it was during these years that Ward's interests finally settled on psychology.

1 See a short sketch of Sir Michael Foster, Professor of Physiology at Cambridge in Chapter 4.

There is a gap of three years in Ward's bibliography after 1883. These years were spent in the preparation of his great Article in the Encyclopaedia Britannica. The publication of this piece is the most important event in Ward's academic life. It was also the beginning of a Wardian era in British psychology.

Before we leave this period however it is interesting to relate the last (dramatic of course) appearance of James Ward Senior in the life of his son. In 1884 Ward's father suffered his final crash. Campbell writes of, "a complete and discreditable business collapse". James was at this time beginning to establish himself in his career, it was around the time of his marriage, and fortunately we are told, "a public scandal was only narrowly averted". Scandal was held at bay mainly because James and his younger brother Arthur agreed to pay the creditors, in yearly instalments, until the end of their father's life (which naturally turned out to be a very long one). They also made him sign a solemn declaration that he would never again, "engage in any independent business undertaking".

G. The Famous Article - 1886

It was very nearly never written.

In 1884 G. Croom Robertson, editor of Mind, who was to have written the article, fell ill. James Sully, who had begun to make his name at London, was asked next, but declined. Thus it was that T. Spencer Baynes, editor of the Encyclopaedia Britannica, looked further afield. Ward writes, in the preface of Psychological Principles, that

Baynes, "chancing to have made my acquaintance offered it to me. I rashly sacrificed the book to this offer and, so it has turned out destroyed one of the dreams of my life".

Ward is referring to the fact that, as early as 1883, Sidgwick and Foster were urging him very strongly to write a text book on psychology. It was indeed "a dream" for him but one which the Article, and the revisions to it which he had to prepare in 1902 and 1908, were to postpone until 1918. Ward himself admitted that the eventual Psychological Principles was, "A belated patchwork, mostly of antiquated rags". His great book is, in fact, more or less based on the article. Only one-third of the book is at all new. The rest is an expanded and revised version of the original 1886 text.

But nevertheless the original Article was a great achievement in itself. It was treated in fact, more or less, as a book. Ward had very little to regret indeed, except perhaps financially.

The psychology of the article is dealt with elsewhere. The influence of it is undoubted. Hearnshaw writes of it thus:

> "His article in Encyclopaedia Britannica was from the outset recognised even by his opponents as, "among the masterpieces of the philosophy of the human mind" (Bain 1886). Its impact on British psychology can be clearly traced for two generations. Flugel writes, "Seldom has an encyclopaedia article aroused so much interest or enthusiasm. It was immediately treated and reviewed as it were a book"."

Campbell writes:

> "In a very short time it established its author's position as the leading British psychologist.

> It was a comprehensive survey of the whole field from the original point of view which Ward had been developing for years. Its effect was immediate and it is not too much to say that it revolutionised the teaching of psychology in Great Britain. Its leading doctrines soon permeated the literature of the subject and often their origin was forgotten".

Perhaps the particular importance of the article (in so far as it is possible to divorce this from the general impact of Ward) is best viewed as five-fold;

1. It firmly established psychology's entitlement to exist in its own right. Even Bain wrote (1886), "This article has the rare merit of being Psychology and nothing but Psychology".
2. It popularised both psychology and psychophysics. Flugel (1933) wrote, "The public and the ordinary student had awakened to the fact that new life was astir in this field which had so long been regarded as being a territory reserved strictly for the philosophically erudite".
3. It dealt a mortal blow to the British tradition of associationism" writes Passmore (1957). This as Hearnshaw (1964 p 136) points out had considerable ramifications. Most notably he claims this "mortal blow" did much to keep, "British psychologists ..... almost wholly unsympathetic to behaviourism in any of its forms", for many years to come.
4. It brought firmly into British thought the influence of the continent. Ward was not swept away with idealism, as were Bradley and Green.

But Ward did combine what remained of associationism with the ideas of Lotze, Brentano, Leibnitz and Fechner, among others.

5. It emphasised the biological viewpoint, highlighting the possibilities for psychology raised by the evolutionary thinking of Darwin and Spencer.

The article was a turning point in British psychology. It was also a turning point for Ward for it established his academic reputation.

H. Ward's Successful Years 1886 - 1925

Ward had married a Miss Mary Martin in July 1884. They had become engaged the previous Winter, following a year in which Ward's health had been, once more, bad. He had in fact suffered an attack of Typhoid in 1882, from which he nearly died, and which adversely affected him for these two years. Miss Martin had attended Ward's lectures in psychology at Newnham College and by 1884 was a lecturer there. Recovered from illness, married and having made his mark, Ward's last 39 years were markedly less eventful than had been his first 43. Indeed they could hardly fail to be so.

Honours came easily to him after the Article. "Up to 1894", he tells us (Ward 1918 p Vl), "I had continued systematically at Psychology, as far as new duties allowed. But in 1894 I became engrossed in other subjects ..... ".

This turn of events had much to do with his being twice appointed a Gifford Lecturer. Firstly in 1894 he was appointed at Aberdeen and secondly at St. Andrews. The Aberdeen lectures entitled Naturalism and Agnosticism were delivered in 1896 - 8 and published in 1899. The St. Andrews lectures were published as The Realm of Ends; or Pluralism and Theism in 1911.

The Gifford Lectures were yet another reason for the delay of Psychological Principles. Work was not resumed until 1913. He received honorary degrees from Edinburgh in 1889 and later from Oxford. He was appointed to the British Academy, the New York Academy, the Danish Royal Society and the Institut de France. In 1902 and 1908, with very little enthusiasm, in fact under legal pressure, he revised the Article for the 10th and 11th editions respectively, of the Encyclopaedia Britannica. In 1897 he was elected to the chair in Mental Philosophy and Logic - a post he held until his death on March 4th 1925. His last major work was his 1922 A Study of Kant on which he embarked after publishing Psychological Principles. The latter part of his academic career was thus, from the time of his first appointment as a Gifford Lecturer almost entirely concerned with philosophy. His psychological system progressed very little from its original (1886) standpoint.

I. A Miscellany

"In academic politics, as in political affairs generally Ward was on the liberal or progressive side. He was a Liberal by tradition as well as by conviction and his convictions inclined to the advanced wing of his party": Thus writes Sorley (1925), who also mentions that, "College and University business" occupied "a good deal of his time".

For instance in 1882 Ward became secretary to Trinity's main executive body, the College Council.

Later in his life, like many other progressives, he joined the fast growing Labour party. He was always on the side of the oppressed, remembers Sorley, but his non-conformity prevented his embracing the more dogmatic aspects of Socialism.

Late in his life Ward's interest in ornithology and nature, which he had developed in his ample free time before joining the architect's office, achieved expression. At one time owls, herons, seagulls, snakes, hedgehogs, tortoises and cats inhabited his house and garden. His collie dog Jan became a legend. She attended, entirely on her own, Ward's important Sidgwick Memorial Lecture. She listened ardently to her master's every word. She was also an enthusiastic, and uninvited, guest at the marriage of Ward's eldest daughter.

There are, of course, very few people still alive who can recall James Ward. One of them is R.H.Thouless whose PhD was examined by Ward. He recalls him as a rather awe-inspiring figure - a "formidable man" in fact. He had a marked tendency, recalls Thouless, to discuss psychology during lectures on metaphysics. However although interested in psychological experiments he did not actively participate or assist at practical classes.

A near contemporary of Thouless, Bartlett, has left similar memories writing (1937 (1));

> "Ward was tall, spare with a magnificent forehead eyes and nose. He was, suitably with his psychological outlook, crammed full of activity ..... In the laboratory he prowled about ..... ill at ease ....

more than a bit disgusted."

Perhaps a last tale about Ward might be told - for it illustrates his (understandable) failure to keep up with events late in his life. Ward was an examiner for Cyril Burt's doctorate on intelligence testing. He is reported, by Thouless, to have said that he found it "very promising work" but that he "hoped the author would go on to something of psychological importance." It is strongly rumoured that he did.

However despite these later events Ward remains a vital figure in the Cambridge School. He left behind him a host of affectionate and heavily influenced pupils including McDougall, Stout, Thouless and Bartlett. We will come across the influence of Ward again and again in the course of this work. For his stamp was firmly impressed upon Cambridge psychology from the very start.

CHAPTER 4

WARD'S FIGHT TO ESTABLISH PSYCHOLOGY AT CAMBRIDGE; 1877 - 1897

A. Introduction

This chapter attempts to outline the story of how psychology first became involved in the institutions of Cambridge University. The prime combatant, James Ward, has already been described in Chapter 3. The social, intellectual and philosophical background to this story has been described in Chapters 1 and 2. Only one element is missing and that is a short description of how Ward formed his firm and fortuitous alliance with Henry Sidgwick, Professor of Moral Philosophy and Sir Michael Foster, Professor of Physiology. Both of these men saw the need to establish psychology at Cambridge to house the new developments, in their disciplines of, respectively, mentalistic philosophy and psychological physiology. Both men were committed to the liberal views of the "Revolution of the Dons" described in Chapter 2 and both were formidable allies in Ward's struggle.

B. Henry Sidgwick and Michael Foster

Henry Sidgwick was a staunch ally of Ward, as well as a close friend, from Ward's arrival at Cambridge in 1872 until his own untimely death in 1900. Born in 1838, Sidgwick had a distinguished academic career at Cambridge, becoming President of the Union in 1861.

He served for several years as a lecturer in Moral Sciences before being elected Professor of Moral Philosophy on November 1st 1883. He became one of the first intake of representatives to the Board of General Studies

in 1882. In this position he represented the interests of the Board of Moral Sciences until 1899 - an unusually long time. Even more importantly he served on the Council of the Senate between 1890 and 1894. Significantly it was in 1891 that the first money for psychophysical apparatus was made available by the Council!

Sidgwick's eventful career is outlined in full by A.S. and E.M. Sidgwick (1906). He was one of the leading academic politicians of the time and in fact in 1885 very nearly stood as a Liberal M.P. He was a member of the Metaphysical Society - described in Chapter 2. Politically, Sidgwick like Ward, was (for those times) an extreme liberal. At Cambridge he fought strenuously for the expansion of the University; for the opening out to greater numbers of pupils the privileges previously available to the rich few. But most of all he led the campaign to admit women to Cambridge. Sidgwick was a founder of the first women's college, Newnham. His wife, a sister of Lord Balfour, sometime Vice Chancellor of the University, was one of the first lecturers at Newnham and its Principal from 1896. Thus Sidgwick was an exceptionally able, well informed, well connected and influential Cambridge figure throughout the 1880's and 90's.

His interest in psychology stemmed from two areas. The first was his close relationship with James Ward, whose career as we have seen, Sidgwick did much to promote. Sidgwick was very much in sympathy with Ward's notion of enriching Mental Philosophy with the use of psychophysics. The second, and probably the most important for Sidgwick

himself, was his interest in psychical research. Sidgwick was a close friend of F.W.H. Myers (no relation to C.S. Myers) who was of course a pioneering figure in British psychical research. Sidgwick played an important role in founding the Society for Psychical Research of which he became the first President. His particular interest was in the possibility of thought transference but his most lasting contribution to the field was the famous Census of Hallucinations which he largely organised for the Society in 1892. This census covered a sample of some 17,000 people!

Sidgwick's commitment to psychology is further illustrated by his attachment to the journal Mind. He took over Bain's financial responsibility for this publication in January 1892. In 1900 he was largely responsible for setting up the Mind Association as a foundation to maintain the journal.

It is unthinkable to claim that Sidgwick foresaw the future experimental psychology as we know it. He was however at the very centre (with Ward) of two outgrowths of philosophy to which scientific methods were soon to be applied. Letters (see Sidgwick and Sidgwick 1906) clearly show that he wanted experimental methods to be applied to both psychical research (which indeed he did himself) and to the sensations, that is psychophysics (which he wanted Ward to carry out). Sidgwick was suitably placed and well able to promote these developments.

Sir Michael Foster was, as we shall see, Ward's second great ally. While Sidgwick pressed psychology's claims from the Moral Sciences Foster headed the second prong of the attack from the direction of the Board for Geology and Biology. Hearnshaw's words (p 75) are more than adequate for our purpose here, he writes:

> "The beginnings of physiology in Cambridge date from 1870 when Michael Foster (1836 - 1907), who for some years had been assisting William Sharpey at University College, London, was persuaded to migrate to Cambridge, as the result of the establishment of a Praelectorship in Physiology at Trinity College. The principal movers in bringing about this new development were George Henry Lewes and 'George Eliot'. Though Foster himself was not an active research worker he was outstandingly successful in attracting and stimulating others, and he played the major part in establishing physiology as a scientific discipline in this country. He was one of the founders of the Physiological Society in 1876, and the first editor of the Journal of Physiology which began to appear in 1878. His "Text Book of Physiology" (1877) superseded all other British textbooks. The brilliant achievements of the Cambridge school of physiology are largely due to the sound foundation which Foster laid in the single room in Trinity College which in 1870 served him both as laboratory and lecture room. Not all these achievements, of course, were relevant to psychology, but in the period before the First World War two lines of work, on the autonomic nervous system and on the conduction of the nervous impulse, were fundamental contributions to the foundations of physiological psychology, and as we shall see later Cambridge psychology itself was on one side the child of Cambridge physiology."

Foster was elected to the Chair of Physiology in 1883. From this position he was to become especially useful to psychology. Most notably, as we shall see, he enticed Rivers to Cambridge in 1892 and eventually provided him

with a lectureship and experimental facilities. It is fortunate indeed that Foster had such a broad view of his subject. It was a favourite quotation of Myers to declare Foster held that, "the whole subject of comparative religion forms but a small part of Cambridge Human Anatomy".

Among the most important work which Foster stimulated was that of Gaskell and Langley. They performed pioneering work on the structure and function of the autonomic nervous system. But Foster was also responsible for stimulating the work of Gotch, Lucas and Adrian on the conduction of nervous impulse. Sherrington and Head (to whom we return later) also studied in Foster's department in their early years. Foster's interest in psychological physiology can hardly be doubted. He made a substantial contribution to psychology in this country not only through stimulating three of the leading physiologists to aid psychology in Head, Sherrington and Adrian but also in the political struggle to which we now turn.

C. The First Psychology Lectures 1875 - 1881

The first time that psychology came into contact with the unyielding institutions of Cambridge University seems to have been in 1875. It was in this year that James Ward presented his fellowship dissertation to Trinity College. As we have seen it was entitled "The Relationship of Physiology and Psychology". The novelty of the subject matter and of its implicit proposals threw his assessors into some considerable disarray. Fortunately, the thesis was accepted. Thus began a forty year period of uphill struggle for those who, like Ward, sought to make psychology an independent and accepted part of the Cambridge scene.

Ward's dissertation caused such consternation mainly because it marked a considerable and radical departure from the traditional viewpoint which placed psychology firmly in the grip of philosophers. The work of Weber and Fechner was an example of how methods, equipment and techniques of observation more commonly associated with 'hard' sciences such as physics and biology could be applied to matters then subsumed under philosophy - a strictly non-experimental discipline.

Ward's argument clearly had profound implications for the institutional futures of philosophy and psychology. Later it will be seen how very difficult it was for the more reactionary members of the senate to accept psychology in its own right. Right up until 1912 when the laboratory was finally opened there was considerable opposition along the lines that psychology did not exist and that its subject matter was either philosophy or physiology and that there was nothing in between.

In May 1877, as we have seen, Ward returned to Cambridge full of enthusiasm for the physiological work he had seen and taken part in at Leipzig in Ludwig's laboratory. It was at this time that his philosophy teacher Henry Sidgwick began to take an active interest both in Ward and in the founding of Cambridge psychology.

Through the direct intervention of Sidgwick Ward began informal lectures in psychology at Trinity in

1878.[1] These were certainly the first lectures in the subject to be given at Cambridge and certainly amongst the first to be given anywhere.[2,3] Ward spent three years giving various informal courses, working at physiology and at tutoring. In 1881 formal recognition at last came to psychology at Cambridge.

It came in the form of including psychology as an examinable subject in the Moral Science Tripos. Sidgwick obtained the subsequent lectureship for Ward. The "Reporter" for this year reveals that Mr. James Ward was to give two lecture courses on "Psychology" and a further one on "Psychology and Psychophysics". For the following

1 There exists a note from Ward to Sidgwick in the Trinity College Archives. In it Ward thanks Sidgwick for his help in securing his temporary tutorship and expresses his hope that Sidgwick's moves to secure him a lecture-ship will prove successful - otherwise he says that he will have to contemplate departing from Cambridge to seek employment elsewhere.

2 The papers on which these lectures were probably based still exist. They are in Ward's own handwriting and are lodged in the archives of the Cambridge Psychological Laboratory. The papers are, in the main, consistent with Ward's later views. The earliest is dated Spring 1875 and is entitled Notes on the Psychical Unit. The paper discusses Ward's famous theme of psychical unity and the active self.

3 Of course it is arguable that Bain, Croom Robertson, Wundt, James, Brentano and even Lotze had been lecturing long before Ward. But this depends on the thorny question of the definition of psychology. However, Ward was prominent in incorporating psychophysics within a psychology course.

years Ward's lectures consisted of two courses in psychology - a considerable advance on "Mr. Levin's" two courses of 1877 - 8, on "Mental Philosophy from Des Cartes (sic) to Spencer". In February 1881 the Reporter notes that Ward's courses were to be examinable - the first examinations in psychology were to be included in the Tripos in 1883. A "Psychology" paper was to be one of six papers for Part I and an "Advanced Psychology and Psychophysics" was to be one of the six papers in Part II. The recommended books were those of Bain, Hamilton, Spencer, Taine, Carpenter and Calderwood. The list displays a notable absence of German influence. Ward's importance in importing German studies can thus hardly be overstated.

But parallel to the struggle to institute exams, courses and lectureships for psychology a battle was also going on for psychological apparatus. To this we now turn.

D. The First Psychological Apparatus 1877 - 1891

Ward, with Dr. John Venn[1] first applied to the University for a grant for psychophysical apparatus in 1877. They repeated the unsuccessful outcome in 1879, 1886 and 1888. It was not until after a 14 year struggle which was both heated and complex that Ward received the princely sum of £50 in June 1891. The earliest applications

1 John Venn had been a Lecturer in Moral Science since 1862. He later became President of Gonville and Caius College and co-authored, with his son J.A. Venn, the first part of Alumni Cantabrigiensis - a complete record of all Cambridge Students up until 1950. Venn was also a candidate for the Chair of Mental Philosophy and Logic to which Ward was appointed in 1897 - which caused some acrimony between the two. (See letter from J.N. Keynes to Ward in 1897 - Trinity College Archives).

are referred to by Bartlett and Myers in 1913 when they lodged short histories of the laboratory in its archives on the occasion of its opening. They are also noted by Campbell (1927) and by Mr. Shipley in the Council of the Senate (Reporter 1909). We are told that the application was turned down largely out of a general conservatism towards such an innovation and also because of religious scruples. Bartlett refers to objections to those who would "insult religion by putting the human soul in a pair of scales". These early applications of 1877 and 1879 do not receive coverage in the Reporter, suggesting that they did not progress all that far. However, all the later ones are well documented and from the verbatim accounts of the proceedings of the Council of the Senate it seems highly likely that these accounts of reaction and religion holding up progress are not far from the truth.

In fact the case of these 1877 and 1879 attempts to buy apparatus is an interesting one. For although they are widely quoted in the literature the documentary evidence for their existence is very sparse.

In the 1870's, lectureships, courses and the day-to-day functioning of academic work were the province of the various Special Boards of Studies. Here we are especially concerned with the Special Board of Moral Science (responsible for philosophy), and the Special Board for Biology and Geology (responsible for physiology). Until 1882 these boards were directly responsible to the Council of the Senate - the effective governing body of

the University. Then with the expansion of the University, a General Board of Studies was set up to mediate between the Special Boards and the Council.

There is absolutely no mention whatever of Ward and Venn applying for "psychophysical apparatus" either in the minute book of the Board for Moral Sciences or in the proceedings of the Council of the Senate. These records are still extant and show no sign of damage or deletion and thus the only conclusion to be drawn is that Ward and Venn's efforts were conducted at an unofficial level. The earliest documentary evidence available is a pamphlet, reproduced here,(1) which was issued by Venn and Ward on May 6, 1886 addressed to "Members of the Moral Science Board". This occasion seems to have been the first on which Venn and Ward enlisted the official support of the Board for Moral Science and this attempt, as we shall see, was also taken up by the General Board and thus is preserved for posterity in the Reporter.

What is beyond doubt is that Ward and Venn were initially unsuccessful in 1877. It was in this year that Harvard granted William James $300 to start a demonstration laboratory and Wundt set up his first demonstrations at Leipzig in advance of the final institution of facilities in 1879. Meanwhile Cambridge began its 35 year long struggle to set up its first laboratory. Thus it was that the lead in experimental psychology passed to Leipzig and not Cambridge.

The next episode in the quest for apparatus began in 1886. The Reporter for June 1891 (p 432) tells us that in November 1886 Venn and Ward made a request for

(1) See Plate 3 following page 72.

£100 to equip a psychophysical laboratory. The Special Board for Moral Sciences agreed and pressed the General Board for finance. They were fully supported by the board for Biology and Geology. The request was refused by the General Board. In 1888 as part of its routine review of expenditure the Moral Sciences Board repeated the application for £100. Again they received support from Biology and Geology, in which Foster was prominent. Again they were denied.

On May 15th 1891 the claim for £100 was, once more, put forward. The same two boards supported Ward and Venn and by now Biology and Geology were proposing the establishment of a lectureship in "the Physiology of the Senses including Psychophysics". This latter claim was not entertained but the General Board did agree to press the Senate for £50 on June 9th 1891. The General Board stated that because room was now available in the new physiological laboratory, "a sum of £50 paid from the Common University Fund be placed at the disposal of the Special Board for Moral Sciences for the purpose of instruments needed for research and demonstrations in psychophysics".

The minutes of the Council of the Senate for June 23rd 1891 record; "On the report of the General Board of Studies on the purchase of instruments for psychophysics, Professor Sidgwick said that the expenditure had received the approval of the Financial Board". Thus Ward and Venn's fourteen year period of wrangling was finally over.

But the fight for proper facilities for psychology was in reality only just beginning.

E. The Growth of Lecture Courses in Psychology 1881 - 1897

As we have seen Ward was appointed a full lecturer in Moral Sciences, mainly to teach psychology, in 1881. It was then that Ward began to teach the U.K.'s first examinable course in psychology.[1] The twenty years which followed this appointment were marked by a steady growth in output of psychology lectures, the extension of examinations and the arrival at Cambridge of Johnson, Stout, McDougall, Rivers and Myers.

In 1881 - 2 Ward extended his two lecture courses in "Psychology" with a further one in "Psychology and Psychophysics". For the next ten years he continued to give three courses in "Psychology" supplemented with two and sometimes three courses in "Metaphysics" emphasising Kant, Reid, the Mills and Spencer. In 1886 - 7 G.F. Stout was made a fellow of St. John's thus starting a celebrated tradition of psychologists at that college. Stout in this same session began to give courses on "Advanced Psychology" and on the "History of Psychology". Stout had in fact arrived at St. John's in 1879 and had been one of Ward's first pupils. This pattern was continued until the next expansion which was to come in 1888 - 9.

Meanwhile, in 1883, the General Board obtained Senate approval for the institution of the Chairs in Physiology (filled immediately by Foster) and Pathology (filled in 1884). The board also stated its opinion that a chair

[1] But see footnote (3), page 55.

be created in "Mental Philosophy and Logic" in not more than two years. Something of the Senate's antipathy to progress in Mental Philosophy and the related topics of psychology and psychophysics is indicated by the fact they refused to act on this recommendation for no less than 14 years. It was not until 1897 that the Chair was finally instituted and Ward appointed to it. Even then this was only achieved after an offer from Sidgwick to lower his own salary by £200 per annum to supply the required stipend (see CAM Collection Prof. of MP and Logic)[2]. Sidgwick communicated this offer in writing to the General Board saying that he wished to avoid the loss of good candidates from the University.

This affair caused a considerable furore. Firstly there was a considerable fly-posting and pamphleting campaign against the appointment, led by James Porter, the Master of Peterhouse. In the Council of the Senate there was heavy criticism of the proposal. The inevitable Dr. Mayo[1] stated that, "all Philosophy was Mental", and furthermore that, "Logic was an awful subject worsened only by rhetoric". Other critics expressed alarm at the precedent set by Sidgwick's offer. However the proposal was passed by the Senate by 128 votes to 71.

1 Venn in Alumni Cantabrigienis describes Dr. James Mayo as a chaplain to various parishes in the Home Counties. He was by all accounts "extremely eccentric". He was invariably present at important Senate meetings. Another of his eccentricities was to take Tripos examinations at frequent intervals when he would invariably achieve a first. He was an implacable and persistent opponent to all things psychological.

2 For details of CAM Collection see Appendix 2.

The elevation of Ward to a Chair, on January 16th, 1897, was an important step. In the ensuing years Ward was able, as a member of the General Board, along with Sidgwick, Foster and Keynes[1] to play an important role in furthering psychology.

All this time Ward and Stout's lectures continued in the same vein until new regulations for the Moral Science Tripos were formulated in 1888 - 89. The nature of the psychology offered for examination is recorded at some length in The Reporter for this year (p 939).

Recommended books now included Sully's Outline of Psychology, Bernstein's Five Senses of Man, Bain's The Emotions and the Will and Ward's Encylopaedia Britannica article. Subsidiary approved books were those of Dewey, Hoffding, Lotze and Spencer.. It will be seen that this revision of the 1881 regulations meant a rather more eclectic approach as well as including, for the first time, Ward's own work. The syllabus is summarised below.

1. Standpoints and methods; Relation to Physics, Physiology and Metaphysics.
2. Analysis and classification of studies of mind. Attention, consciousness, impressions, feelings, retentiveness.
3. Sensation and Perception; Intensity, complexity, quality of sensations. The physiology of the senses. Activity and passivity of the mind.

1 John Neville Keynes was chairman of the Board of Moral Sciences for many years. He was father of the famous economist John Maynard Keynes and a staunch ally of psychology.

4. Images, imagination and judgement. The psychological theory of categories.
5. Thought comparison. Society and the individual.
6. Emotions, higher sources of feeling, aesthetic, intellectual, social and moral.
7. Voluntary action; Will, practical reason, pain and pleasure.

This syllabus, reflecting the characteristic analytical psychology of Ward and Stout was adopted and used throughout the 1890's and in fact until the next major revision of 1910. This next revision was to reflect the growth of interest in practical experimentation which grew under Rivers and Myers.

But to return to 1890 we find a rise in the output of psychology courses to match the new regulations. Ward began at this time to give two courses in "Psychology", one in "Psychophysics" and three in "Metaphysics". Stout, meanwhile began to give three courses in the "History of Philosophy". From 1891 Ward was also able to demonstrate to his pupils the machinations of his much sought after psychological apparatus. In 1896 Stout left for Aberdeen, taught at Oxford from 1898 until 1903, and eventually took the Chair of Logic and Metaphysics at St. Andrews. Thus he began the export trade in eminent psychologists which Cambridge was to make very much its hallmark in the years to come.

The next step came in 1893. It came from Foster and the Board for Biology and Geology and marked a further step

towards placing psychology among the sciences rather than among philosophical studies. W.H.R. Rivers was asked by Foster to come to Cambridge and lecture on the physiology of the senses. But his post was not recognised for a further four years.

In a letter on May 19th 1897 the General Board reported that it was now supporting an application from the Board of Biology and Geology. They passed on to the Senate the Board's letter which stated that, "a great need exists for a special course of study in the physiology of the senses adopted for those who are studying, psychology (e.g.for the Moral Sciences Tripos) as well as for those who are studying physiology". This letter had in fact been written and presented to the General Board on December 7th, 1888. In March 1889 Moral Sciences had intimated their support. So it was, nine years later that the post was actually created, the General Board acknowledging the "repeated applications".

Luckily the Senate agreed, "That a university lectureship in physiological and experimental psychology ..... be established for a period of five years, dating from October next, and that the stipend be £50 a year." The voting was 38 to 5 but opposition came, as usual, from Dr. Mayo. On this occasion he organised an extensive fly-posting campaign. His pamphlet stated, of the phrase, "Physiological Psychology"....."I do not see how it adds anything to 'Psychology' and that is a word which may mean anything or nothing". His opposition continued when the motion came before the Council of the Senate.

Mayo's opinions, and those of other speakers illustrate two rather interesting points. Firstly they show a blank incomprehension of the existence of any area between philosophy and physiology. Secondly they demonstrate the possibly damaging effects of Ward's friendship with Sidgwick - which apparently led many to confuse psychology with Sidgwick's interest in psychical research. But Mayo, as ever, was the most extreme and entertaining of psychology's opponents. He declared that, "a fresh lecturer was unnecessary". There were already Professors of Physiology, Pathology and Zoology, he went on, "if these could not give lectures on the physiology of the organs of sense they did not adequately perform the duties of their chairs." If the topics to be covered were those of Moral Science why could not philosophy deal with them, added Dr. Mayo. If this was not so then surely the University was proposing a "psychology of dreams, second sight and apparitions. These were also sheer nonsense or approached witchcraft".

The council were unconvinced and W.H.R. Rivers was swiftly appointed to the post.

F. The Arrival of Myers and Rivers

The advent of Rivers marked the beginning of a new period in the history of Cambridge psychology. Until this time Ward had arguably been the leading figure in psychology in the U.K. let alone in Cambridge. The injection of new blood in the shape of Rivers and later Myers was to mark an increasing emphasis on laboratory psychology. The shift away from armchair psychology,

which Bain had started and which Ward had taken as far as psychophysics was soon to accelerate.

For Rivers and Myers held that there was very little indeed that was psychology which could not be experimented upon. Ward's view, as we shall see, was very different. It is ironic indeed that Ward who did more than anyone else to set up psychology in Cambridge on an experimental basis should believe that in the end psychology was not a basically experimental discipline.

So 1897 seems a suitable juncture at which to close this section which has essentially shown Ward's leading role in beginning experimental psychology at Cambridge. This is not to say, indeed it is far from saying, that Ward ceases in 1897 to play an important role. He remains very important especially in founding the British Journal of Psychology and the Psychological Laboratory in Downing Street. Rather it is that what happens in the years 1897 - 1922 becomes increasingly due to the efforts of Rivers and Myers and decreasingly to Ward. If 1877 - 1897 was the era of Ward then 1897 - 1922 is the era of Rivers and Myers. Thus the story of practical developments in psychology at Cambridge is taken up again in Part 3.

CHAPTER 5

JAMES WARD'S PSYCHOPHYSICS, PSYCHOLOGY AND PHILOSOPHY

A. Introduction

It is with some considerable justification that Boring (1950 p 489) calls Ward a "difficult philosopher-systematist". An adequate account of his writings could all too easily take up far more space than is available here. For they range over the whole of psychology and philosophy, span some 50 years and many are turgidly and obscurely written.

Luckily two points of cardinal importance for psychology emerge from the mass of his work. The first was his realisation of the possibility for expanding the work of Fechner in psychophysics. The second is more complex. Ward totally rejected what he saw as the elements of physiological determinism in the thought of Bain and indeed of William James. However he also rejected the traditional Associationist account of a series of states which somehow "knew themselves". This left him with a problem. If consciousness is not an epiphenomenon and if it cannot be explained by the traditional Associationist account, what alternative is there? To answer this Ward merged what he saw as the best of German Idealism with the best of British Associationism. The self became the active source of human behaviour. However within Ward's system mental events were causally connected to each other. As Johnson has written (1925) Ward was "ready to admit to a non materialistic form of determinism".

This doctrine was popularised par excellence in Stout's textbooks. It is a point of view, claims Joynson (1974) which psychologists, with the notable exceptions of the

Gestaltists and Bartlett, have all too often ignored. However we return to this argument later.

The scheme adopted here for the presentation of Ward's thought is no doubt one of many possible approaches. It is one chosen however to illustrate the growth of his coherent system. This, in the end, encompassed psychophysics, psychology and philosophy - as well as putting physiology firmly in what Ward adjudged to be its place.

I have chosen, as far as possible, to present a chronological account of Ward's work while making a special effort to illustrate the original sources of his thinking.

In February 1914, Ward was presented with a portrait of himself by his pupils colleagues and friends. He said, "What I am two men have made me; Hermann Lotze and Henry Sidgwick." Lotze was the spur and initiator of Ward's thought. Sidgwick trained him in the British tradition and, as we have seen elsewhere, made most of what follows possible.

B. Ward's Psychophysics; The Influence of Fechner

Ward's first published papers (Ward 1874 (1) + (2)) were on "Animal Locomotion". He was, however, never serious about a career in physiology, even though he excelled in the subject. Ward's desire to return to Germany and to Ludwig, in 1876 and again in 1879 probably stemmed from his recently discovered enthusiasm for psychophysics.

Gustav Theodor Fechner in the Germany of the 1850's began to wrestle with the same problems that were later

to plague James Ward. Fechner made his name as a brilliant physicist. In 1839 a personal crisis and severe illness curtailed his promising career at the age of 38. He emerged from his crisis trained in science and yet deeply religious - the same difficult and paradoxical combination that faced Ward in the 1870's.

Fechner's eccentric and varied career has been ably outlined by Boring. Writing under his now famous nom de plume of Dr. Mises, Fechner, from 1841 onwards, began to argue for the mental life of plants. Now, as we have noted in Chapter 1, materialistic science was held in high regard in the Germany of this time. His nom de plume was designed to deflect from himself the inevitable notoriety he knew his views would evoke. By 1848 he had begun to formalise his views. He asserted, in Leibnitzian fashion, the identity of mind and matter and assured the world that the entire universe could be readily viewed from the point of view of its consciousness. Dr. Mises was in other words propounding panpsychism. By 1851 he had worked out his programme to demonstrate the truth of panpsychism - through psychophysics. As Boring points out, the importance of this programme for Fechner was to resolve, to his satisfaction, the religion- materialism debate. Ironically the importance of Fechner's psychophysics to psychology is of a completely different character.

As an ex-professor of physics Fechner can hardly have been unaware of the principle of the conservation of energy. Thus, he thought, if he could demonstrate

that a lawful relationship existed between physical and mental energy surely he would have demonstrated the essential identity of the two?

Thus Fechner set enthusiastically and productively to work. The result was the publication, in 1860, of the Elemente der Psychophysik. Fechner had worked with great care and precision. He realised that he could not measure the intensity of sensation direct. So he developed instead the three famous methods which are his greatest legacy to psychology. These were:

1. The method of the just noticeable differences or j.n.d's.
2. The method of right and wrong cases or constant stimuli.
3. The method of average error, adjustment or reproduction.

His main experimental findings were:

1. That the intensity of a sensation (S) had a strict logarithmic relationship to the intensity of the stimulus (R). Thus Fechner's law, as it is now known, states that $S=K \log R$ where K is a constant.
2. That the j.n.d. was proportional to the absolute size of the stimulus, this is now known as Weber's law as it is what E.H. Weber had previously discovered in his studies of notably, touch.

Fechner's findings proved a fruitful research field. He had succeeded in setting up an area of experimentation within which findings could be challenged, replicated or extended. There were and are several objections to Fechner's findings such as the validity of adding up j.n.d's. to find the size of a stimulus and the questionable application of the logarithmic relationship between S and R over the whole range of sensation.

But Fechner did, at the very least, show the possibility of experimentation on the senses and the possibility of establishing mathematical relationships between stimulus and sensation. He thought he had verified panpsychism. He had not. For the simple fact is that his findings are acceptable within most mind-body theories.

Ward, no doubt, came to hear of all this during his spell with Lotze, at Gottingen, in 1869. Lotze was interested in the psychophysical debate which followed in Fechner's wake. By this time the debate involved Volkmann, Helmholtz, Mach and even Wundt. There is little doubt that Ward would have been introduced to Fechner's work (along with the physiology, phrenology and metaphysics that Lotze revelled in) before returning to Britain.

In 1875 Ward's fellowship dissertation was presented to Trinity College. Entitled The Relation of Physiology to Psychology it was really An Attempt to interpret Fechner's Law under which title it appeared, edited, in Mind 1876.

Ward's enthusiasm for psychophysics continued for many years.

Plate 3 (see adjoining page) shows Ward and Venn's unsuccessful application for psychophysical apparatus in 1886.

This application shows how Ward's interests in experimental psychophysics had expanded. By this time, as well as wishing to extend Fechner's work he was showing interest in: Helmoltz's investigation of musical notes and vowel sounds; the nature of colour induction and contrast; various tactile and optical illusions.

Perhaps more interestingly he was also wishing to follow up the work of Donders a Dutch physiologist. In 1868, Donders had begun experiments on choice (or complex - as we now call it) reaction time. He had also arrived at the breakdown of the process into choice, discrimination and reaction. This field of study was expanded later in Wundt's laboratory by Ludwig and Lange from 1888 onwards. Ward wanted to do this in 1886.

When Ward was, eventually, successful he obtained among other things a Helmholtz double siren and an early Hipp chronoscope. Ironically, by this time Ward was engaged in his systematic psychology and tending towards philosophy. He never published any psychophysical findings. Nevertheless Ward had imported Fechnerian psychophysics into Britain. If he had been allowed his money in 1877 British psychophysics might too have proved a fertile field. In the event it was not until Rivers and Myers effectively took over Cambridge psychology that Cambridge finally grasped the nettle and applied Fechner's methods to the study of the senses.

PLATE 3

The earliest firm documentary evidence of any attempt to obtain psychological apparatus at Cambridge. However, even this, Venn and Ward's third effort to persuade the University, proved unsuccessful.

[*For Members of the Moral Sciences Board.*]

Some years ago, when this Board was called upon by the General Board of Studies to state in what respects further University provision for the study of the Moral Sciences was desirable, it was agreed that the Board should apply among other things for a sum of money to be expended on apparatus for psychophysical experiments. This recommendation of the Board was, however, afterwards withdrawn. But it was withdrawn mainly because at that time the need of additional teaching in the department of Political Economy was still more urgently felt, and the Board was of opinion that the precise teaching required was only to be obtained by the foundation of a Readership in Political Economy. Afterwards this scheme proved impracticable: now it is unnecessary. Under these circumstances it seems not unreasonable to suggest that the Board should reconsider its original proposal to seek a grant for psychophysical apparatus; especially as the time contemplated in the inquiry of the General Board is now at an end and the University is about to receive larger contributions from the Colleges.

It may not perhaps be altogether superfluous or impertinent to describe briefly the kinds of experiments contemplated. They fall practically into three classes:—(i) *Experiments connected with Weber's Law*, or psychophysical experiments in the narrower sense, i.e. to say experiments to ascertain generally the range of sensibility and the conditions of sense-discrimination as regards the intensity, quality, duration, and localization of the impressions compared.

(ii) *Experiments to ascertain more exactly the constituents of sense-perception under various circumstances.* Helmholtz's investigation of musical notes and vowel sounds may serve as an illustration of this class. The nature of colour induction and contrast, and the various tactical and optical illusions also belong here. Such simple instruments as the stereoscope and stroboscope are of great service in making patent the complexity of apparently simple perception. As regards both (i) and (ii), indeed, the chief requisite is simple apparatus for demonstrations. Those who have to teach in other branches of science would never consent to fall back on diagrams or descriptions of phenomena which could be readily exhibited by means of not very expensive apparatus. Original investigations under these heads could not with advantage be undertaken by ordinary students of Moral Science. But if more facilities were afforded in the University for such work it might be reasonably expected that some of those who begin with the study of experimental physics or physiology might be interested in questions of a psychophysical kind. At least such is the case elsewhere.

(iii) *Experiments to ascertain the time occupied by the simpler mental processes* are the most important of all. Since the first experiments of this class were made by Donders in 1868 above a score of memoirs have appeared; most within the last few years and many of them by pupils of Wundt and under his direction. A good deal of light has been thrown upon the processes of attention and association by this means and much more seems possible. The old objection, so often and so vehemently urged, that psychology can never become scientific because it is without any means for exact analysis or measurement is thus to some extent overcome. The time occupied by a given mental process may be in general regarded as some index to its complexity: on the other hand the more we can learn of the complexity of a process in other ways the better we shall be able to interpret its time-measurement. Thus different methods of inquiry may be expected to supplement each other. No doubt only very simple acts admit of this treatment; but it is precisely in regard to such that direct observation is most at fault, and largely for want of any exact knowledge of these more elementary facts that psychology as a science has progressed so little.

Quite recently a Psychophysical Laboratory has been founded in the Johns Hopkins University, Baltimore, and a number of papers published. Some of these which are to be found in the last and current volumes of *Mind* or the papers in Wundt's journal *Philosophische Studien* may be referred to as shewing the kind of work to be done.

An original outlay of from £75 to £100 and the use of a quiet well-lighted room would make it possible to begin.

J. VENN.
J. WARD.

May 6. 1886.

c. The Origins of Ward's Psychology; His Definition of Psychology

As we shall see later, Ward was not slow to draw upon the centuries of experience which British Associationism afforded him. But the standpoint of his psychology is a thoroughly foreign one. Ward's psychology of the active self bears a distinct family resemblance to the act psychology of Brentano. Thus as Hearnshaw (1964) puts it "What Ward essentially did was to replace the traditional British analytical approach .... by a 'Leibnitzian' point of view". This view as Allport (1955 pp 12 - 16) puts it, maintains that the person is the source of acts and not a mere collection of sensations, perceptions or even actions.

Hermann Lotze has a remarkable place in the history of psychology. He taught both Ward and Brentano as well as Stumpf and G.E. Müller. He had no disciples yet he had these four very diverse and grateful followers, all of whom are probably better known than Lotze himself.

A persistent theme in Lotze's system, as he freely admitted was that an act of faith, in God, was required to bridge the gap between "the world of mechanism" and the "world of values" as Passmore (1957 p 50) puts it. This satisfied neither Ward nor Brentano. Psychology From an Empirical Standpoint, Brentano's exposition of his "Act Psychology" appeared in 1874, after considerable personal help from Lotze. Ward was back in Germany, in Leipzig, in 1876 and again in 1879 so it is quite possible, indeed it is highly likely, that Ward read Brentano's work before developing his own system.

Brentano's solution to Lotze's problem (which in modern parlance we might call the mind-body problem) was to argue that every mental act necessarily implies and involves the material world. Ward's formulation is a little different but the essence of his position is the same. Ward maintained that the terms Subject and Object were meaningless on their own. Each, he claimed, implied the other. Furthermore, all our knowledge consists in our experience of the interaction between the two. Thus the individual's experience is the cornerstone of Ward's system. Ward and Brentano both place the active, experiencing self at the centre of their psychology. Both men thus hoped to transcend the traditional mind-body problem and base their psychology upon this solution.

What then, in detail, is Ward's standpoint? The best discussion of this is provided by Ward himself. Called "The Definition of Psychology" the discussion appeared at the beginning of each edition of his Encyclopaedia Britannica Article and his Psychological Principles. It also appeared as the very first article in the very first edition of the British Journal of Psychology in 1904. In his "Definition" Ward makes some timeless remarks which are just as relevant to today's psychology as they were at the time of writing.

He begins:

> "Everybody can tell in a general way what Psychology is about; In fact there is perhaps no science the subject matter of which can be more clearly and promptly set forth in popular language and for practical purposes."

Yet, he goes on, although descriptions of "normal mental processes" are readily available for historians, biographers and even "physicians and educationists" "the moment, however, that we attempt to pass beyond approximate definitions and determine exactly what the term "mental processes" means or implies we find ourselves beset with serious difficulties."

It is, says Ward, an inadequate consideration of this problem that has plagued the history of psychology. For, he continues, "the theory of knowledge and the theory of conduct, raise questions which depend in large measure for their solution on the conclusions we reach concerning this problem." The history of psychology, he writes "was first unduly 'objective' and then unduly 'subjective'; it is only now beginning to shew signs of maturity in a due balance of the two; the fundamental concept of the first period was Life, that of the second Mind, that of the third is Experience. To understand this last we must consider the other two in turn."

This Ward proceeds to do. Aristotle, he claims, founded the tradition based on Life. This tradition regarded psychical facts from the outside. The mind or soul was viewed only through the way it guided the physical body. Rather as the "modern physiologist" regards vision as the function of eye the function of soul was the result of the complex needs of a body as highly developed as that of man. But, as Ward concludes his account of the Aristotelian tradition;

> "Even with our present knowledge we could learn little more about intellectual processes if we attempted to begin by studying the brain

> than if we began by studying the heart .... It is not thinking as a process in the individual mind so much as thought as a universal product that Aristotle considers; but when - upon occasion - the individual, as distinct from the universal, aspect of thought is foremost with him then biological or physical analogies are apt to obtrude. What we miss in Aristotle is a clear recognition of what we now call consciousness as the central feature of all psychical facts. Regarding these facts from the outside rather than from within..... he failed to find an adequate unity for the diverse functions which he described; he had to rest content with the biological conception of an organism into which however he infused a strong teleological colouring".

In other words the behaviour of an organism, for Aristotle, is the sum of its component organs' interactions with each other and with the environment. The final cause of all this activity is what we call the <u>soul</u>.

Turning to Descartes, Ward writes;

> "We are at the opposite extreme. The connexion of body and mind, the cornerstone of Aristotle's construction, was the chief stumbling block in the way of Descartes' advance and has remained a perplexing problem even to our own day. The hazy materialism, into which the Aristotelian psychology had developed in mediaeval times, Descartes banished once and for all by the new definitions which he gave of matter and mind. Both were substances and therefore essentially distinct ..... and between these there was no common term and there was no natural connexion."

The first problem this brings about, says Ward, is that of the false division of <u>outside</u> and <u>inside</u>. In the Cartesian scheme a conscious event is something <u>inside</u> us. And yet how did it get there? What <u>is</u> the relation of mind and matter? Descartes resorted to

the omnipotence of God to relate the two realms. Ward could not accept this solution for reasons which we discuss in the following sections. Ward also pointed out that the over subjective emphasis on mind militated against the biological aspects of psychology. In those post Darwinian times Ward was keen to include such considerations in his scheme. Ward ends his discussion by writing;

> "These two problems - the relation of body and mind and the reality of external perception have continued to vex philosophic thinkers from Descartes' day to our own nor will they cease to trouble us till dualism is laid to rest."

One feels considerable sympathy for Ward in rejecting these doctrines. He is surely championing, in psychology, what Joynson (1974) has since called "the layman's understanding". He has after all rejected biological materialism and the conscious automaton theory as well as the view that our minds somehow act under little or no bodily influence.

Ward's answer to these age-old problems is to formulate his "Standpoint of Psychology as individualistic". He writes;

> "Psychology then we define as the science of individual experience - understanding by experience, not merely, not primarily, cognition but also, and above all, conative activity or behaviour ;" (My emphasis).

He does this because, he argues, all we know is our own experience of being, of acting, striving, knowing and feeling in the world. Our experience implies both subject and object and thus it is experience that should form the basis for not only psychology but epistemology as well.

For knowledge, in the natural sciences, is he says; "knowledge as it is for all ..... the product of many minds". Knowledge in psychology is discovered in "the standpoint of the living subject in intercourse with his special environment". While in the natural sciences "the characteristics of individual environments are in general ignored".

We have spent so long here describing Ward's standpoint because it is this, more than the intricately argued edifice he placed upon it, which has survived the years and had a good deal of influence. Through Stout's textbooks Wardian psychology was the standard introduction to the subject in Britain for nearly fifty years. As we shall see in Part 4, Ward's standpoint was a <u>direct</u> inspiration for Bartlett. As Bartlett put it (1925 (1))

> "There was a ..... part of psychology which had for him a particularly irrestible attraction: the study of memory. Here if anywhere, was to be found actual evidence of the integrity and independence of mind, and of the indissoluble character of individual experience."

William McDougall, however outdated he may seem now, was a major figure in the development of British psychology. He was taught by Ward and wrote in the introduction of his <u>Outline of Psychology</u> (1923)

> "I venture to regard my book as an endeavour to carry to its logical conclusion that critical rejection of the "mosaic psychology" which has been a main theme of the psychological writings of Messrs. James Ward, F.H. Bradley, Dawes Hicks and G.F. Stout."

At a greater distance Allport (1955) in "Becoming" presents a strong, and on his part thankful, case for the

retention of the Leibnitzian tradition within American studiesof personality. This tradition,he writes,(which included in the 1950's Goldstein, Angyal, Lecky, Revers and Sinnot might today be extended to those like Ma low, Rogers, Jourard and MacMurray) owes much to the Gestaltist influence in America as well as that of McDougall.

However the influence of Ward's standpoint in Britain is clearer. Myers and Bartlett both embraced Ward's central doctrines of the activity of the self, "the indissoluble character of individual experience" and, crucially, his bitter attacks on the atomism of Wundt. It is true that Ward's standpoint may be flawed and not over encouraging towards an experimental approach. But it did provide the philosophical backbone of the Cambridge School. As such, it is probably the one major factor in maintaining the independent stance of Cambridge, and indeed British psychology up to the Second World War.

D. Ward's Psychological System

Ward's standpoint for psychology was more or less fully developed by 1881. In unpublished lecture notes, dated 2nd May 1881, he presents much of the discussion we have just reviewed. But in one illuninating passage he writes "The position taken up by J.S. Mill and Bain seems to me a sound one ..... when we add to it to the recognition of the subject," (My emphasis). Ward's attack of typhoid restricted his productivity in the early 1880's but his finished psychological system finally appeared in the Article in 1886. The system changed very

little between this initial appearance and its final one in Psychological Principles in 1918.

Let it be said from the start that, although Ward's Article appeared in the same year as his third appeal for funds for experimental research, there is hardly a mention of any experimental findings in the whole work. Ward's Psychology is a masterpiece of its kind. Every page is closely, concisely and carefully argued. The system is devoted to the logical consequences of his standpoint. Ward's psychology, as Stout put it in 1926, "is always concerned with some concrete individual experiencer - who feels, knows and is active in the way of attending, striving and willing."

Fechner's outer psychophysics was to Ward an examination of the flow of activity between Subject and Object and as such had no place in his psychology (see p 103). Fechner's 'inner psychophysics', which would, if Fechner had got round to it, have examined sensation - sensation relationships and Donder's work on choice both should, logically, have been involved in Ward's work. Indeed by the time Psychological Principles finally appeared as a book in 1918, logically, there was some 30 years of experimentation which could at least have been referred to. The omission of all this work is a sure indication of Ward's lack of enthusiasm, later in his life, for psychology. Wardian Psychology is thus, in reality, a product of the 1880's and not the early 1900's. By the time it appeared for example Myers had published his Introduction to Experimental Psychology which lucidly put many of Ward's

philosophical meanderings onto a laboratory basis. Thus Ward's system is not of the greatest importance for the development of the Cambridge School. The account which follows is therefore more 'for the record' than to illustrate any influence it may have had.

Ward retained, in his system, the three part division so much favoured by the Associationists. Experience could be divided into Cognition (knowing) Conation (willing) and Emotion (feeling). Experience exists in the "presentation-continuum" - a notion Ward derived from Lotze's theory of local signs. This continuum is "plastic" in that it exists in a primitive form at birth and then develops and grows. As Ward puts it (p 29) "Experience is the process of becoming expert by experiment." A "psychoplasm" handed down genetically the "stuff of mind" or Erlebnis.[1] This notion (see next section) later involved Ward in his panpsychism.

However attention is the ultimate explanatory factor. Attention can be distributed cognitively and conatively, producing sensations which are sensory or motor 'presentations' respectively. Such presentations are assimilated into the 'presentation continuum' thus changing it. The principles of assimilation depend on the traditional laws of contiguity and association (so important to the Mills and Bain) but even more on attention. Feelings exist in the company of attention and presentation and thus attention can be guided by cognition, emotion and feeling.

The notion of the presentation continuum is one of a growing, changing psychical body. It grows as does

[1] This term is defined by Ward (p.57) as: "What is common to cognitions, feelings and conations - viz that they are all events experienced or 'lived through'." Each element or function in Erlebnis, he writes, "though analytically distinguishable ..... never actually exist apart."

nervous tissue by differentiation. "Where there is psychical plasticity there is neural plasticity" is how Stout (1926) interprets Ward on this point. Writing of the gradual differentiation of the continuum Ward claims (p 49);

> "It is quite impossible for us now to imagine the effects of years of experience removed, or to picture the character of our infantile presentations before our interests had led us habitually to concentrate attention on some and to ignore others. In place of the many things which we can now see and hear not merely would there then be a confused presentation of the whole field of vision and of a mass of undistinguished sounds, but even the difference between sights and sounds themselves would be without its present distinctness, the nearer we approach to a total presentation having the character of one general continuum in which differences are latent. There is then, in psychology, as in biology, what may be called a principle of 'progressive differentiation or specialisation' ".

Ward's system is best outlined by Stout (1926) where a far more detailed account than is possible, or desirable, here is available. In ending this section it is perhaps pertinent to point out the similarity between Ward's "presentation continuum" and Bartlett's "schemata". Both account for the distinctly individual nature of experience by affording assimilation to the residue of past experiences. Both provide accounts of the influence of past knowledge, interests emotions and desires in cognitive processes. Both put forward the idea of a constantly changing active memory system as opposed to a "store-house of past experiences" as Bartlett was later to put it. But that is as far as Ward's system concerns us here.

E. The Philosophy of James Ward

Introduction

The account below owes much both to Murray's clear and thorough account, The Philosophy of James Ward (1937) and to the various articles in the memorial edition of The Monist (1926) which was devoted entirely to a consideration of Ward's work.

Ward's major philosophical works were his Gifford lectures (Ward 1903, 1911) which were prepared and delivered in the years 1896 - 1898 and 1907 - 1910. There were however various essays on philosophical topics in his earlier years. But, as with Ward's psychology, there were no fundamental changes within his system during his life and his philosophy can usefully be treated as a coherent whole - as indeed Murray does.

Ward was a trained Congregationalist minister as well as a trained physiologist when he began his career in Moral Science at Cambridge. Thus he faced, in a peculiarly personal way, one of the burning intellectual questions of his time. This was the problem, mentioned in Chapter 2, of the competing claims of science with its associated materialism and religion, which in Britain was enrolling the philosophical support of German Idealism, largely through Bradley and Green at Oxford. Murray writes;

> "In 1873 Ward had posed himself the problem: How is a disciple of modern thought to be religious? And it was because Lotze showed himself acutely aware both of the demands of exact science

> and of the claim of moral and religious values that the young Ward found in him his philosophic father".

For Ward, Murray continues, "Religion was a fact of experience and any philosophy worth its name had to find a place for it". Yet the accomplishments of science were also well known to Ward. Thus recognition of its methods and claims had also to find a place in his philosophy. Ward's solution was to deny supremacy either to materialism, as Bain would have him do, or to idealism, as Bradley mentioned he should. Thus Passmore (1957) discusses Ward thus: "Ward was a devoted Lotzean, whose philosophy incorporated science as one of its constituents" (p 82). Elsewhere he claims, "Lotze's philosophy is what came to be called an 'Ideal-Realism' - understanding by 'Realism' the view that the way things happen is determined by mechanical conditions and by 'Idealism' the view that things happen in accordance with a plan or in order to fulfil an ideal purpose". Ward owes his general approach to Lotze and the label 'Ideal-Realism' is not an inappropriate one for his philosophy either.

Panpsychism

Ward was profoundly dissatisfied with the two great philosphical movements of his time. Ward, and Murray, use the family name of Absolutism for theories leaning towards idealism. Ward (1904 (3)) saw these as deriving from Descartes and encompassing Hegel, Kant, Spinoza and his contemporaries Bradley and Green. Similarly they use

the term Naturalism for theories with a scientific and/or materialistic bias. These Ward (op.cit.) saw as a tradition originating with Aristotle and encompassing some of British Associationism - but especially Bain and the growing idea that physiological or material explanation was applicable to psychology. This convention is followed here - it has its drawbacks but the scheme is convenient for the argument presented below.

Ward saw philosophy as having two major aims, speculative unity and closeness to experience. While Absolutism excelled at the former it failed miserably at the latter and vice versa for Naturalism. Murray considers that Ward's philosophy is an attempt to solve this traditional dilemma, and although it is one which founders nevertheless it has enriched the subject.

Ward argued against both the conclusions and methods of Absolutism. Its conclusions are contrary to experience he claimed. For Absolutists must hold that all objects are in some way transformed or transfigured aspects of the Absolute. This is contrary to our experience of the world as infinitely variable, discrete and diverse. Furthermore Absolutism leads too easily to the Cartesian dualism of mind and matter. Quite apart from the problem of communication between the two spheres only one of them can be held to be fully real. Hence one sphere is reduced to "a mere 'appearance' or 'epiphenomenon'; or alternatively that neither is fully real, since both are appearances of the Absolute." (Passmore 1957 p 83) None of these alternatives satisfied Ward.

He was no more satisfied by their methods. By "beginning from above", as Murray puts it, Absolutism surely tried to explain the known by the unknown. This procedure destroyed the pluralistic conception of the objective world, it insisted that the Subject can only be active and lastly destroyed the unity of Subject and Object in which consists our whole experience.

Ward was no more impressed with Naturalism. Firstly the materialism so often implied and sometimes claimed by scientists rejected the existence of God. This was contrary to experience. Secondly Naturalism tends to miss the essentially purposeful or teleological nature of the world. It had discovered evolution yet, Ward claimed, could not fully explain it. Naturalism was even further from explaining human purposive action. Other arguments raised by Ward included Naturalism's fragmentary and analytic approach which was, in the end, unsatisfying to the genuine inquirer after truth.

But there were two main sticking points for Ward. The first was that Naturalism could give no account whatever of God. If it attempted to do so it merely lapsed into dualism raising the old problems mentioned above. The second was the scientist's failure (as Ward saw it) to recognise that his observations were happenings in his own mind. They did not give absolute access to the truth, they were abstractions from reality. Scientists, he claimed had consistently failed to examine their own presuppositions. If they had, Ward claims, they would have found out that they had no account whatever of

the existence of even their own observations! Naturalism cannot, claims Ward, account for spiritual, mystical or even private knowledge - thus it flies in the face of experience.

Ward saw the problems of these two great traditions as arising from their artificial dichotomy between Subject and Object. Absolutism accepted it without question but could not solve the resultant problems. Naturalism simply failed to account for purpose and mental life in any way at all. So Ward decided to dispose of the cause of all the trouble - as he saw it.

The obvious solution to resort to was the panpsychism of Leibnitz. In taking this direction Ward was as Murray expressed it, sailing "the precarious course between the Scylla of a discrete and somewhat chaotic plurality and the Charybdis of an all-engulfing absolute." Ward attempted to justify his choice by a discussion of philosophical method (see Murray Chapter 4). Here Ward claims experience must be the cornerstone of any system. Before, he says, such emphasis has always led to the problem of the place of mind in the universe. But surely as Naturalism has made great strides by looking at mechanical activity why not invest the mind with activity as well? A further justification for Ward's leap into panpsychism was provided, or so he thought, by the law of continuity. This was initially a philosophical principle propounded by Leibnitz and formed the basis of his own panpsychism. The Law held that nature never makes leaps. The universe progresses only by gradual development and differentiation. It was a sign of the age Ward lived in that this philosophical law had apparently recently received powerful empirical

and scientific support from Darwin.

Ward wished to bridge the gaps between the mechanical and the teleological, between the inorganic and the organic and between mind and matter. Leibnitz had attempted the same task - as indeed, much later, had Fechner. Panpsychism holds, briefly, that activity is the fundamental fact of the universe and that the basic units are "monads".(1) Organic and inorganic matter differ only in their levels of development. Inorganic matter is merely unstratified or undeveloped mind. Teleological action is thus merely the action of substance more highly organised than that capable of only mechanical action. The crucial new elements in Ward's scheme, as opposed to that of Leibnitz were:

1) Ward's "monads" had the capacity for direct interaction - Leibnitz's had not this property.

2) Ward's "monads" were also unlike Leibnitz's, invested with the capacity for free and striving action. This has severe consequences for Ward's system. Leibnitz's system was one of a preconceived harmony and hence tending towards a "mechanical pluralism" unacceptable to Ward's fierce defence of free-will.

Ward's panpsychism does indeed appear to solve some of the problems he faced. It did however lead him into some new difficulties. This is no place to discuss the merits of various mind-body theories (see for this Campbell 1970 Vesey 1964). It will suffice to say here that

(1) Strictly speaking the term "monads" belongs to Leibnitz's theory and not necessarily to all versions of panpsychism.

panpsychism is a somewhat questionable solution to the problem.

The particular problems with Ward's version lie in two areas. Firstly he does not seem to have achieved his own aims. Murray records that Ward, in his first Gifford Lecture, stated, "we should never let go of the concrete," and, "keeping strictly to the concrete and historical everywhere we find variety and diversity." Panpsychism is surely too unifying a concept for Ward. Murray (along with William James) suggested that panpsychism was a needless complication.

On the contrary, I intend to argue, as Ward did until the end, (Passmore p 84) that panpsychism was essential for him. Pluralism could not solve, for him, the problems of the subject-object dichotomy on which his psychology and psychophysics were based. Ward's approach to both these latter topics is rooted deeply in the notion of integrated and universal activity that only panpsychism can embrace. Furthermore his psychophysics, as Fechner's did, depends heavily on mental and mechanical activity being of the same nature. Fechner as we have seen, thought he had verified panpsychism through psychophysics. To throw out panpsychism would have meant throwing away much more than Murray seems to realise.

The second difficulty was a purely logical one. Ward defended the concept of free-will tooth and nail. To do this he was forced also to accept the self-limitation of God. Now Ward had, as we have seen only arrived at

panpsychism by noticing that Naturalism, although producing apparently teleological theories, such as entropy and evolution, could not explain them. Hence as only spirit can be teleological Ward invested the whole universe with it. Thus Ward's solution itself ends in a dualism for there are still teleogical and non-teleological elements in the universe. But Ward had to leave this, slightly unsettling, element within his system. For he could not accept the alternatives which were either Omnipotent Deism (which ruled out free-will) or Occasionalism (which also sometimes ruled out free-will).

Murray wrote on this point that Ward's importation of the Law of Continuity to solve the problem of his "two apparently opposing criteria of philosophy: speculative unity and closeness to experience .....could not bridge the gulfs in experience between matter and mind, the mechanical and the teleogical, the organic and the inorganic, and it could not do this because of the dualism in things due to the self-limitation of God.....So Ward does not seem to be able to have it both ways."

Conclusions

Thus ends this short section on James Ward's difficult and all-embracing philosophy. In his system he hoped to allow for scientific progress. Mental and mechanical activity were both open to scientific study. A self-limiting God guided the activity of the free and striving monads, the highest developed of which, human beings, had become able to interact among themselves and with God himself.

Murray attempts to summarise Ward thus;

> "The pre-supposition of philosophy is the pre-supposition which leads to pluralism in the first instance and then leads away from it, that is the pre-supposition of positivism or radical empiricism. It is the pre-supposition that the universe as we find it, is as we find it at least to a certain extent, whatever further examination may show it to be like. And this very positive approach to the universe shows it as embodying in it elements making towards unity and organisation and oneness. Positivism leads to idealism. Radical empiricism leads beyond pluralism and beyond itself. Pluralism does not sustain itself. Thus it is that we find Ward developing a kind of empirical monadology which differs somewhat from its historical ancestor with its rational pre-suppositions for all philosophy and is a monadology of interaction and co-operation and freedom."

But Passmore has succeeded in 'rooting out' our main point of interest;

> "Materialism, according to Ward, can make nothing of the striving, valuing individual: for to understand the individual, he thought, we must make use of that category of purpose which the materialistic discards. But if we suppose the environment too is purposive, spiritual then Ward tells us all difficulties in relating man to his environment will vanish. This does not mean Ward hastened to point out, that we must abandon the idea of scientific law: we come to see however, that a law is a product of mind, of our way of dealing with our environment..... Ward approaches the mind as a biologist: mind, he says, is active desirous; experience is the process of'becoming expert by experiment'. (Passmore pp 82, 83, 84. My emphasis)

Bartlett was later to view Psychology as a biological science, with the mind having a certain life of its own, meriting its own study. Myers also shared this view and, as we shall see, in his hands Ward's panpsychism

became rather less mystical and more akin to modern Identity Theory.

Ward was not a major philosopher. His aims were perhaps too various and conflicting. His influence on Cambridge psychology though, as we shall see, is undoubtedly huge. However, he was among the teachers of Russell and Moore. When one recalls that they continued Ward's fight against Idealism and that Identity Theory is, like Ward's, an attempt to transcend the traditional approaches to the mind-body problem perhaps one should not too quickly dismiss Ward's impact.

It should also be remembered that if the account of Ward's philosophy given here seems incomplete, fragmentary or even contradictory then there is good reason. For there are all these elements in his work. Murray makes repeated references to Ward's omissions while Passmore rather amusingly writes that with Ward, the main difficulty is "to decide what he really meant to say on the issues of central philosophical importance. It is clear enough that he hoped to leave room somewhere in his philosophy for individuality and God, diversity and the Absolute; it is not at all clear how."

F. Ward's Academic Contribution - A Summary.

Ward's academic career was full of paradox. He started with psychophysics. By the time his apparatus had arrived he had lost interest. He was by that time fully occupied in his non experimental Psychological Principles. By the time this book appeared, his version

of a pure non experimental psychology was almost totally outmoded and Ward himself was almost totally the philosopher. Nevertheless he is a major figure in the history of British psychology. Perhaps his contribution is best summed up point by point as below:

1) He along with James Sully initiated British interest in psychophysics.

2) He brought the first psychophysical apparatus to Cambridge.

3) His vehement onslaughts (see Ward (1893 (1) 1904 (11)) on the atomism of Bain and Wundt left a lasting mark on British psychology. This as Drever (1968) put it, "staved off the simpler S-R theories" and made it far easier in the 1950's "for British psychology to adopt the recent information processing theories".

4) His adaptation of Lotze's local sign theory into that of the presentation-continuum is a predecessor of Bartlett's theory of schemata.

5) He firmly established the Leibnitzian tradition in Britain.

In many ways Ward stands in British thought where William James does in American. Ward of course has never received the attention that James has. But many factors

have militated against this. Ward was in the van of an unpopular cultural movement in Britain. The religious reaction against psychology in America (as in Germany) was not nearly so strong. James also received greater institutional support. Furthermore James wrote lucidly and fluently - being the brother of a popular novelist! Ward, on the other hand, was pernickety and stunted in his writing style, and had no Jamesian gift for the telling illustration. Even apparently simple points became bogged down in a sea of logical propositions - often Latinised into the bargain.

Yet both men tried to set up experimental laboratories in the same year. Both produced grand-scale psychological text books from which, with the exception of the James-Lange theory of emotion little or no experimental work emerged. Lastly both men had no time for elementism sharing an insistence on the activity proposed by Brentano and the functional character of consciousness suggested by evolutionary theory.

PART THREE

RIVERS AND MYERS; CAMBRIDGE PSYCHOLOGY

BECOMES ESTABLISHED 1897 - 1922.

CHAPTER 6

RIVERS AND MYERS - AN INTRODUCTION

The years 1897 to 1922 are the years in which the Cambridge School became firmly established. In 1897 all that existed of Cambridge psychology was James Ward's grand system, his dabblings in psychophysics and Rivers' early work in his post in the "Physiology of the Senses." By 1922 Cambridge had a large, well equipped, purpose built, psychological laboratory which ranked as one of the best in the world. Psychology had made inroads into courses on medicine, anthropology and education. Over 80 students used the laboratory every term and psychology had three full time staff Bartlett, Sprott and MacCurdy - instead of half of Rivers' post. Courses and advice were sought by and given to local hospitals, the Industrial Fatigue Research Board, the armed forces and teacher training colleges. By 1922 psychology also had its own Board of Studies instead of being sandwiched between moral science and biology.

In other words by 1922 Cambridge psychology had firmly established its right to exist as an independent academic discipline as well as indicating its utility in various outside areas. Rivers and Myers were largely responsible for creating this new respectability.

It was also during the years 1897 - 1922 that the Cambridge School took on its eclectic and practical

character. From James Ward the school inherited its emphasis on the active self and its aversion to atomism. From Rivers and Myers the school took on its medical, anthropological and industrial colourings. From Myers in particular Cambridge psychologists inherited a positive phobia of all embracing theories and systems and their reliance on concrete data and real life situations. As Hearnshaw puts it (1964 pp 173, 174)

> "He was a broadly competent experimentalist. At a time when psychology was beginning to break up into 'schools' Myers held steadily to an empirical 'middle of the road' position, seeking for steady advances both in experimental and applied psychology, never over-theoretical, though, as his later papers showed, his work was founded on a deeply thought out position."

The eclectism and practicality of Rivers and Myers had much to do with their common background in anthropology and medicine. Both men went on Haddon's Torres Straits expedition which marked the beginnings of cross-cultural psychology in the United Kingdom. Both men were fully qualified medical doctors. For as Bartlett 1969 puts it;

> "Myers' approach was based essentially upon his clinical medical training. When he adopted a psychological career, it was entirely natural for him to combine exact and controlled methods of investigation with a recognition that these must be combined with a diagnosis of individual temperament and character and of the social influences at play upon them."

This general practical approach combined surprisingly

well with the philosophical views of Ward. For, as Bartlett continues of Rivers "like Ward and Myers, though in his own way, he insisted on the recognition of individuality and character as original constituents on response even in the most carefully controlled psychological experiment."

But there the comparison between Rivers and Myers ends. Rivers for most of his life was essentially the man of ideas. Rivers published very nearly 150 different items between 1888 and his premature death in 1922 - very nearly five a year. The range of his interests was truly breathtaking. He studied medicine and hysteria before turning to the psychology of vision. After the Torres Straits he was an anthropologist, during the war a psychotherapist and afterwards an interpreter of Freud and an ethnological theorist.

Myers on the other hand, was a natural organiser and innovator. When he died three of the country's leading psychological institutions, the B.P.S. the N.I.I.P. and the Cambridge Laboratory, owed a very great deal to his efforts. The latter two arguably owed more to him than to anyone else.[1] The case of Charles Myers is a fascinating one of the impact on a discipline of an able enthusiastic, organiser and initiator of activity.

Thus Part 3 of this thesis attempts to relate and assess the impact on British psychology of these two fairly remarkable men, similar in their background, very close friends and yet, in the end, of very different academic interests. The next two chapters present short

[1] The work of A. F. Shand, the first Secretary of the B.P.S. was also very important in the society's early days.

biographies of Rivers and Myers. Then, Chapter 9 relates the story of the consolidation of psychology at Cambridge which occurred under their guidance. The last chapter in Part 3 is devoted to an exposition and assessment of their academic work and attempts to weigh up their lasting impact on British psychology.

CHAPTER 7

W. H. R. RIVERS - A BIOGRAPHY

1864 - 1922

(Plates 4, 5, 6 and 7, in that order, appear on the following pages).

PLATE 4

W. H. R. Rivers 1864 - 1922

Arguably he was Britain's first experimental psychologist.

PLATES 5 (TOP) AND 6

Plate 5 shows Rivers with William Brown and Elliot Smith at the Maghull "Shell Shock" hospital. McDougall, Seligman, Pear and Myers were also, at different times on the staff.

Plate 6 shows Rivers (far left) in the Torres Straits with (left to right, Seligman, Haddon, Ray and Wilkin. Myers and McDougall were also on the expedition.

PLATE 7

"A human experiment in nerve division". Rivers (right) is operating on Henry Head.

Dr. Rivers. Dr. William Brown. Dr. Elliot Smith.
Military Hospital, Maghull 1915.

Torres Straits, 1898

A. Early Years and Family Background 1864 - 1892

William Halse Rivers Rivers was born, near Chatham, Kent on March 12th 1864. This account of Rivers' life owes much to Myers' 1922 tribute to his friend which formed his presidential address to the Psychology Section of the British Association. Rivers was intended to give the address but died shortly beforehand. He is probably the least well known member of the Cambridge School. Yet of the four he probably had the most remarkable personality and his intellectual span was truly astounding, even by post-Victorian standards.

He came from a respected family with a rather interesting history. His paternal grandfather, Lieutenant William Rivers, served with Admiral Lord Nelson as a midshipman on The Victory. He was present, allegedly, at Nelson's famous death scene. Indeed Lieutenant Rivers acquitted himself with some courage, being wounded in the mouth and having his left leg shot away. W.H.R. Rivers later followed his paternal family's strong naval tradition - though for rather different purposes. Rivers' father however was a pastor. He was H.F. Rivers M.A. who had trained at Trinity, Cambridge. He became vicar at St. Faith's, Maidstone. Thus, Rivers was raised in rural Kent.

Rivers' background was just as interesting on his mother's side. His mother, Elizabeth, nee Hunt was the sister of Dr. James Hunt, the founder and first President of the Anthropological Society. As a leading figure of

this Society, which was founded in 1863 and which later became the Royal Anthropological Institute, he fought for recognition of Anthropology much as his nephew did, later, for Psychology. Fittingly when W.H.R. Rivers died, in 1922 he was President of the Royal Anthropological Institute, which nearly sixty years earlier his uncle had founded.

Rivers was educated firstly at Brighton. Then from 1877 to 1880 he was sent to the Tonbridge School with a view to his qualifying for Cambridge. This plan was suspended for a year when Rivers contracted what Myers (1922) calls enteric fever and Head (1923) calls typhoid. During this year Rivers appears to have become attracted towards medicine. Thus in 1882 he entered St. Bartholomew's Hospital, London. In 1886 he received his Bachelor of Medicine becoming, despite his year's illness, the youngest ever to receive this degree from St. Bartholomews. Thus qualified, he voyaged to the Far East for a year as ship's doctor making his first acquaintance with an area which was to occupy a major part of his academic career.

In 1888 he returned to England, obtained his full M.D. and took a hospital appointment at Chichester. The following year was spent as a House Physician at St. Bartholomews. However during this appointment he became interested in "nervous diseases" and eventually secured himself a post at the National Hospital, Queen Square, London. Here, in 1891 he had his first meeting with Henry Head - the precursor of thirty years' friendship and fruitful

academic co-operation. Head had just returned to Queen Square after a spell in Prague with Ewald Hering. Thus Rivers first came into contact with the new experimental psychology of the continent but especially with Hering's work on colour vision. When we remember that Rivers' interests were turning towards "nervous diseases" and we also note that he had been studying Spencer's work we can see that Rivers' course towards psychology has its beginnings around this time of his first meeting with Head. At Queen Square, Rivers studied under Hughlings Jackson and also assisted at the pioneering brain operations of Victor Horsley. The "Queen Square Neurologists" are ably described by Hearnshaw (1964). The importance of, especially, Jackson's work was not lost on Rivers. Jackson was a pioneer of the more modern study of cerebral localisation. He put neurology onto a far more scientific basis which eventually began to overshadow phrenology and the far more abrupt localisation theories of, notably, Flourens. Jackson was also prominent in incorporating evolutionary theory within physiology and neurology. Using this background he postulated differentially developed levels of nervous organisation, from pure reflex to purely voluntary action. The highest levels were 'protected' in simple normal functioning - but were the first to disappear after brain damage. In these ideas were the seeds of the later work of both Sherrington and Head. To Rivers the close links of physiology and neurology to nervous disease had been clearly demonstrated. As indeed had the possibility of a physiological and psychological study of sensation - through Hering's work.

In 1892 Rivers spent the Spring and Summer in apparently studying "philosophy, psychology and mental disease" (Head 1923) and attending the lectures of Eucken, Zielen and Binswanger. In his diary he recorded (rather ambiguously) "I have during the last few weeks come to the conclusion that I should go in for insanity when I return to England and work as much as possible at psychology." He did precisely this.

B. First Incursions into Psychology. 1892 - 1898

On his return Rivers became Clinical Assistant at the Bethlem Royal Hospital. Soon he began lecturing on psychology under James Sully at University College, London and also began to lecture on the psychological aspects of mental disease at Guy's Hospital. But Rivers' real chance to make a name for himself came in 1893 when Michael Foster plucked him from this situation and installed him at Cambridge. As we have seen in Chapter 5 Foster had become aware of psychology's possibilities through the efforts of Sidgwick and Ward. Now he gave Rivers a brief in the "Physiology of the Senses and Experimental Psychology" which was formalised in a lectureship of that name in 1897. Rivers was soon forced to give up his teaching in London. In October 1893 he took up residence at St. John's College continuing that College's psychological tradition started by Stout. Soon he was off again, to Heidelberg this time where he studied under Kraepelin for a time. Most interestingly he became acquainted with Kraepelin's work on fatigue, work curves and the effect of drugs on bodily and mental operations.

Thus he came into contact with the third of his major early psychological interests. These were mental disease, the senses, especially colour vision, and lastly fatigue and drugs. His bibliography for his early Cambridge years is indicative of these pursuits. In 1894 he published a review of Külpe's psychology. In 1895 he reviewed Maudsley's Pathology of Mind and published papers "On Binocular Colour Mixture" and on "Experimental Psychology in Relation to Insanity." The following years saw papers on the apparent size of objects, mental fatigue and recovery (including a joint study with Kraepelin) and "The Photometry of Coloured Paper." These papers included the first results to emerge from Rivers' new laboratory provided in the Physiology Department by Foster. Soon the problems of colour vision and space perception came to preoccupy him. These two themes predominate his psychological work - which as we shall see more or less concluded in 1908. In the years prior to 1900 he was thoroughly immersed, when possible, in his encylopaedic article on vision for Schafer's Textbook of Physiology. In this article he performed the valuable service to British psychology of reviewing all the previous experimental work on vision. Most of this was of course previously inaccessible, being both in German and for the most part in Germany as well. But we are going too fast. In 1898 occurred the event which led to Rivers' departure from Psychology - the Torres Straits Expedition.

Rivers was invited by A. C. Haddon to head the psychology section of his Torres Straits Expedition. For reasons only hinted at by Haddon's biographer Quiggin (1942)

Haddon wanted to take strict experimental psychologists with him. As Quiggin puts it, "A psychologist was essential. But psychology did not mean 'brain spinning', for which he had a supreme contempt; like everything else it had to be firmly grounded on experiment and his first choice fell on W.H.R. Rivers,"(op. cit. p.96).

Initially Rivers refused, probably because his own work was progressing well and receiving recognition. Haddon them recruited Rivers' two best-ever pupils Myers and McDougall. This step prompted Rivers himself to come along too. Haddon wrote (op. cit. p 97), "When Rivers found that his two best students were going he asked whether, after all, he might come too. Naturally I was very much pleased at this though I own that I felt that the psychological side was rather overweighted. I put direction of the psychological department entirely into the hands of Rivers and for the first time psychological observations were made on a backward people in their own country by trained psychologists with adequate equipment." Thus Rivers became responsible for the instigation of cross-cultural psychology in Great Britain - though as we have seen much of the credit must also go to the foresight of Haddon himself.

In brief, (the topic is covered in Chapter IO as well), Rivers collected sociological data which eventually became his consuming interest, and also investigated the natives' vision. Myers studied music, smell and reaction time while McDougall studied "tactile sensibility and other

observations," (op. cit. p 101). From this point on Rivers' interests drifted steadily towards anthropology. His last experimental studies were performed in the post Torres Straits years but ended in 1908 with the publication of "The Illusion of Compared Horizontal and Vertical Lines" and "The Influence of Small Doses of Alchohol on the Capacity for Muscular Work." This latter paper marked the end of years of, fairly important, work on muscular fatigue which Rivers had carried out with considerable dedication. He had in fact gone to the extraordinary lengths of requiring his subject to abstain for a year from all alcohol and even the caffeine in tea and coffee. However the results of this work are reviewed in Chapter IO.

C. Rivers the Anthropologist 1898 - 1914

Although as we have seen above Rivers did continue his psychological work he had really lost his heart to Anthropology in the Torres Straits. On his return from the expedition, he and Myers sought comparative data in Scotland. In the Winter of 1900 in Egypt he furthered what Myers called "psychological work of the same comparative ethnological character" and what we would now call cross-cultural studies of sensation and perception. His publications for 1899 and 1900 show a pre-occupation with cross-cultural studies and with anthropological method. Then in 1901-2 he made his first expedition on his own to the Todas of Southern India. As Haddon puts it, (1922), "a few years later he made an intensive study of the Todas

of Southern India, and his book (1906) proved how immeasurably preferable scientific method was to ill-trained or untrained observations." Haddon is referring not only to Rivers' applications of psychological methods but also to his contribution to the study of primitive genealogical and kinship systems. Rivers seems to have had a natural talent for what we would now call participant observation. He was remarkably successful in his studies of native customs and social structure. Haddon (op. cit.) continues "Those who have worked with him in the field cannot fail to have noticed how his patience and sympathetic manner with natives enabled him to gain information where another investigator might have failed."

On his return from India Rivers dedicated himself mainly to writing up his data from the Torres Straits (Samoa, Papua, New Guinea, Borneo) and from the Todas. These papers begin in 1901 with a study of the functions of maternal uncles, sons-in-law and brothers-in-law in the Torres Straits. They continue in the following years with "Observations on the Vision of the Uralis and Sholayas" and studies of the funeral customs, kinship, marriage, prayers, senses and even the astronomy of the Todas.

While he was doing all this in the five years from 1902 and 1907 he was busily experimenting, at Head's suggestion, on the cutaneous nerves in Head's arm. They did this in Rivers' rooms at St. John's College on weekends and holidays. The results of this work appeared in 1908 as "A Human Experiment in Nerve Division" which Rivers wrote jointly with Head.

In 1908 Rivers was elected to a Fellowship of the Royal Society and paid his first visit to Melanesia. With

two assistants he collected data from the Western Solomon and Shortland Islands. When he came back Bartlett, who arrived at Cambridge in 1911, recalls, "By the time I knew him he had thrown in his lot with Anthropology. He was completing what he regarded as his magnum opus, the History of Melanesian Culture. He removed himself from almost every other form of activity, and he constantly warned me against getting embroiled in practical affairs in College or University." Indeed the years following Rivers' first visit to Melanesia are marked by papers on magical practices, totemism, kava-drinking, the Solomon Island basket, island names, conventionalism in primitive art and the "Sun-cult and Megaliths in Oceania." This work was however best presented in his History of Melanesian Society. Haddon, who was of course well placed to comment, wrote, in 1922 :

> "The publication of his monumental book....marks an epoch in ethnological research and method. His clear analytical mind enabled him to unravel the genealogies and kinship terms in which he delighted and from which he deduced systems of relationship previously unknown, and also to recover sociological conditions which have now passed away. He found that systems of relationship and many customs concerning marriage, descent and other social institutions, were remarkably permanent under a veneer of introduced civilisation, and that often really valuable data could be gleaned from the most unpromising places. He also showed how certain institutions and customs have arisen, or have been profoundly modified, by the result of interaction between peoples, and he established cultural complexes in Melanesia which can be definitely assigned to various immigrant peoples."

In 1911 Rivers had been elected as President of the Anthropological section of the British Association. When

the war came in 1914 he was, once more, in Melanesia. On his return, in March 1915, he volunteered immediately, as had Myers, for active service. His previous interest in neurology landed Rivers very quickly at Maghull Hospital, the centre for shell shocked soldiers which is described in the next chapter.

D. The First World War and After; 1914 - 1922

At Maghull, Rivers first came into contact with the newly publicised theories of Sigmund Freud. Ernest Jones had founded the London Society of Psycho-analysts in 1913. In the same year T. H. Pear and William Brown, who were with Rivers at Maghull, read papers on Freudian theory to the British Association. Rivers was "greatly influenced, though never swept away, by Freud," writes Bartlett (1969). After the war he published papers on "The repression of war experience," "War neurosis and military training," and "Psycho-therapeutics." His experience at Maghull and later at Craiglockhart Hospital, Edinburgh convinced him of the reality of repression, the efficacy of catharsis and the essentially psychological nature of "shell-shock." Freudian theory also profoundly influenced Rivers' post-war anthropological studies (for further discussion on these two issues see Chapter 10).

Rivers ended the war, from 1917 on, as a consulting psychologist to the R.A.F., attached to their central hospital at Hampstead. During this time he taught himself to fly to the extent even of performing aerobatics so that he might then go on to devise tests to select the

best recruits for flying. The end of hostilities curtailed this new avenue of enquiry.

His three good friends, Bartlett, Myers and Head are all agreed that Rivers returned from the war a changed man. Bartlett writes (1969) "He seemed to have suffered, or achieved, a tremendous shift of interest. Gone were all his prejudices against engagement in practical affairs." Indeed they had for, as Myers reports,

> "He found time to serve on the Medical Research Council's Air Medical Investigation Committee, on its Mental Disorders Committee, on its Miners' Nystagmus Committee and on the Psychological Committee of its Industrial Fatigue Research Board. He served on a Committee, of Ecclesiastical Complexion, appointed to inquire into the new psychotherapy...."

It was a considerable conversion which also changed his behaviour at Cambridge.

He had been elected as a Praelector in Natural Sciences at St. John's in 1919, a post with no formal teaching duties. Yet after the war he sought constant contact with college affairs and took part in and organised countless informal discussion groups. He also organised a group called "The Socratics". This group brought to St. John's leading scientists from all fields as well as leading literary figures, like H.G. Wells, Siegfried Sassoon and Arnold Bennett.

The extent of this conversion and also of his continuing breadth of intellectual activity is confirmed by the fact that in the year of his death he held three quite disparate posts. These were President of the Royal Anthropological Institute, President of the Psychology Section

of the British Association and lastly he was the adopted Labour candidate for Parliament of the University of London. He was also in the last years of his life President of the Folk Lore Society.

His publications in these post war years continued his anthropological writings, although he undertook no new expeditions. But an overriding theme was the assimilation of Freudian concepts within anthropology and psychology.

Rivers died, aged only 58, on June 4th 1922. His death was sudden and unexpected, coming after only a few hours of as Head puts it "acute intestinal obstruction."

Rivers' death clearly made a great impact on Myers (as shown in his Presidential Address to the British Association Psychology Section) and on Bartlett who wrote (1923);

> "On June 3rd last year I was walking through the grounds of St. John's College, here in Cambridge, when I met Dr. Rivers returning from a stroll. He was full of energy and enthusiasm, and began at once to talk about certain new courses of lectures which he proposed to deliver at the Psychological Laboratory during the present year. On the evening of the next day I heard that he was dangerously ill. As I approached the College on the morning of June 5th I saw the flag at half mast. He had,in fact, died in the early afternoon of the previous day. Never have I known so deep a gloom Settle upon the College as fell upon it at that time. There was hardly a man - young or old - who did not seem to be intimately and personally affected. Rivers knew nearly everybody."

E. Rivers' Personality

R. H. Thouless who was at Cambridge in the post-war years recalls Rivers as an "immensely impressive man"

while Zangwill, at second hand, describes him simply as "charismatic." Thouless remembers he was very free and friendly and that the discussion groups which he held informally in his rooms once a week were well worth attending. Rivers was much concerned with Freudian ideas after the War and this was a factor which led him to be a "very great influence" in leading Thouless to an interest in the psychology of religion. As we shall see later he was also a very great influence on the young Bartlett - so great that Bartlett was very nearly lost to anthropology.

Rivers had strict rules on his conduct. He worked hard every morning and was 'incommunicado'. After lunch he was free to dispense advice and discuss matters with whoever wished to see him. He never, never worked on Sundays believing firmly in the necessity of mental relaxation.

Like Myers, Rivers' strongest characteristics were obviously his constant seeking after novelty and his immense energy. The scope of his intellectual activity is truly breathtaking. His dedication and thoroughness are perhaps best illustrated by three things. First there are his four anthropological expeditions to the Far East. Secondly there is his insistence on his subject's year-long abstinence from alcohol and caffeine and thirdly there is his determination not only to learn to fly but to perform aerobatics so as to better understand the psychology of flying.

Perhaps as Bartlett, Myers and Head all point out someone who never met Rivers could never hope to understand how much he had contributed or what an inspiring figure he was to all that knew him.

PLATE 8

Charles Samuel Myers 1873 - 1946

"He built a laboratory, a society, an institute".

F. C. Bartlett.

CHAPTER 8

CHARLES SAMUEL MYERS

1873 - 1946

"He built a laboratory, a society an institute"

(All quotations in this Chapter, unless otherwise attributed, are from the extensive "Personal Record" lodged by Myers with the Royal Society in 1942).

A. Early Days and Family Influences

C. S. Myers was born on March 13th 1873. His early life and background, as he was never slow to admit (Myers 1936, 1942), was a great influence on him.

For a start he was born the eldest of five brothers. Thus perhaps it is hardly surprising to see him so naturally adopting the role of leader and organiser wherever he went. He was also a member of a highly enterprising and successful Jewish business family. So perhaps we should not be too startled to see Myers showing such administrative talent, organisational flair and initiative in his later life.

Charles Myers spent his first years, "in the confines of Bayswater and Notting Hill". Wolf, his father was an affluent and successful clothier, who had been born in Chelmsford in 1842. Esther, his mother, was the daughter of a London wholesale clothier. Myers' Jewish business pedigree is indeed remarkable. His two grandfathers and all four of his great grandfathers, to a man, distinguished themselves in various businesses "in or near London". Myers own share of this family wealth, bequeathed

to him by his father, later largely purchased Britain's first permanent Psychological Laboratory.

In fact Myers always intended that this building should serve as a memorial to his father. At the time however, in 1912, the source of the money was hardly known outside Myers' immediate circle. Thus Wolf Myers the principal benefactor to the laboratory and whose memorial it constituted went unrecognised for many years afterwards.

From his father then Charles Myers gained three things. Firstly, he had the example of an enterprising hard working man who nevertheless found time to devote himself to various causes in the Jewish community. Charles Myers later did just the same. Secondly, as we shall see, he obtained the chance of a broad, wide ranging private education. Thirdly, he inherited a large sum of money with which he provided the wherewithal for a vital stepping stone in the history of British psychology.

Myers himself attributed many aspects of his personality to his mother's influence. But a representative passage comes from Bartlett (1947). "Of one other profound family influence no manner of doubt can be entertained. Charles Myers inherited or acquired those 'remarkable social gifts' which distinguished his mother." Bartlett is here discussing Myers' quite outstanding abilities to handle people. Throughout his later life he showed great talent for organising, leading and persuading people. The founding of the Cambridge Laboratory and the N.I.I.P. were both tasks calling for charisma, leadership, persuasiveness and charm as well as the more obvious requirements of competence in administrative and

and academic matters.

Esther Myers has one other, more tangible, part to play. This was in leading Charles in the direction of music. From the age of 8 he was encouraged to play the violin. He wrote (1942) "I used to rise at about 6 a.m. and practice the instrument daily before breakfast".

Music became a life-long interest; as a subject for anthropological and psychological study; as a relaxation; and as an excuse to flex his organisational powers. In fact he became a talented violinist - a factor which secured his place on the Torres Straits Expedition. Bartlett (1947) wrote;

> "All his life music remained one of his leading interests, and whether as auditor or performer, a deep pleasure at all times and a solace in difficulty. Music went with him into his scientific studies; and some of his investigations into the development of rhythm and tone in primitive societies, into synaesthesia and into the analysis of highly developed musical appreciation, were probably about the best original work of a purely scientific character that he achieved".

Apart from the influence of his parents it is also important not to ignore the nature of Myers' extended family.

Myers seems to have been surrounded by an energetic, rather individual and closely-knit extended family network. Wolf Myers had nine brothers and sisters while his wife had fifteen. His many uncles devoted themselves to diverse and often idiosyncratic pursuits. One became an amateur philosopher and historian, another took to Egyptology while a third founded a firm of solicitors and amassed a famous collection of old coins.

The key to Myers' role in British psychology indubitably does not lie in his academic achievements - significant though they were. Rather it lies in two areas - both of which can be seen developing from his early years. These two are the extent to which Myers was a social animal, always putting people first, last and in between and in his almost manic, restless desire to become involved in new enterprises. These two themes, coupled with his social and organisational skills, will return again and again in this short sketch of Myers' life.

It seems not too far fetched to attribute these qualities to his upbringing within the large, sociable, talented and somewhat eccentric Myers family.

B. School Years

Myers' early schooling took place at two private schools in Bayswater and Notting Hill which in retrospect seemed to him "rather distinguished". Later the family was wealthy enough to send Myers, aged 11, to the City of London School. Here he became quickly attracted to Chemistry.

Towards the end of his time at school he began to excel at the classics, especially, he tells us, in Latin prose and verse. Indeed Myers had done well enough at school to sit for a Cambridge entrance scholarship, either in Classics or Natural Science. What seems to have tipped the balance, for Myers, still only 16, were the first inklings of his desire to practice medicine. In 1942

Myers wrote,

> "My first inklings towards a vocation - medicine - came at the end of my school days. I had a handsome first cousin whom I worshipped as a hero. He took a medical degree and I wanted to imitate him".

So Myers, in order to take medicine, opted to try for an entrance exam to Cambridge in Natural Science. He must have been highly determined on his chosen course - for it meant two things. Firstly it necessitated an extra year improving his schooling in the natural sciences. The City School had not given him sufficient grounding in this area and so Myers took an extra years' training in the "Preliminary scientific class" at St. Bartholomew's Hospital.

The other difficult aspect of this decision regarded Myers' father. Wolf Myers had given his 17 year-old son a completely free hand as to whether he took Classics, Natural Science or joined his father's business. Myers wrote later that this was an extremely "difficult choice" as his father had made no secret of what <u>he</u> would prefer - namely that his eldest son should follow tradition and take over the family business. Nevertheless Myers was free to decide. Under the influence of his cousin, as we have seen, his English master from school and his family's group of "learned friends" it was to the natural sciences he turned.

At St. Bartholomew's Myers tuition was supplemented with private lessons. In the Summer of 1891 he took and passed a Cambridge entrance scholarship and was accepted by Gonville and Caius College.

C. Undergraduate Years 1891 - 1895

The college archives record that C. S. Myers entered Gonville and Caius in October 1891. He was one of 16 Exhibitioners that year - an award worth £20 per annum. His promise was soon recognised and he spent the following three academic years with the elevated status of College Scholar. This was no mere nomenclature, it meant grants of £40, £50 and £50 for the next three years - made from the College Foundation Fund.

Myers spent his first two years studying chemistry, physics, botany, zoology and physiology. Of these topics he became quickly attracted to animal physiology. More out of place was his developing interest in botany which, in 1892, won him the Frank Smart prize.

Myers was also awarded the considerable sum of £27.10s. as a Shuttleworth Scholar in 1895. This scholarship was intended to aid natural science students wishing to specialise in medicine. But animal and human physiology soon became his main interests. Myers puts this down to his tutor Sheridan Lea and also to the Professor of Physiology who was, of course, Michael Foster. Myers became a close friend to both these men - but most importantly to Foster.

Myers' intellect and energy guided him to the rather rare distinction of a double first in the Natural Science Tripos. The first part was accomplished in 1893. The second, which allowed Myers to specialise in Physiology and Human Anatomy, was successfully passed in 1895.

As we have seen in the previous chapter, in 1893, Foster had brought Rivers to Cambridge to teach in the

Physiology of the Senses. Charles Myers was one of the pupils at the first class held by Rivers under this arrangement. Myers later wrote of these classes, "To these subjects as well as to this teacher (who became a very intimate friend) I became greatly attracted".

Another person to become important at this stage of Myers' career was the eminent anthropologist A. C. Haddon. At this time Haddon was fighting his own battle for the recognition of anthropology. He perhaps even faced stauncher opposition than the pioneer psychologists. His story has been told by A. H. Quiggin (1942). Cambridge's apathy towards Haddon is really fairly astounding. Here was a famous anthropologist who in 1888 and 1898, had led two highly productive, respected and successful expeditions to New Guinea and yet for many years he was plagued by financial hardship and certainly only granted the most miserly recognition by the University. As Rothblat (1968) has pointed out, Cambridge was slow indeed to assimilate the new developing sciences around the turn of the century. The case of Haddon and anthropology seems to be even worse than that of psychology.

Nevertheless Haddon and anthropology were able to attract the attention of Myers. (Just as, with Rivers' assistance, the topic was later to stimulate the undergraduate Bartlett). "Fired by my teacher Dr. A. C. Haddon's enthusiasm" wrote Myers in 1936, "I became especially interested in physical and racial anthropology". This was no fleeting undergraduate fad.

Myers' first published paper appeared, in 1896, in the Journal of the Anthropological Institute and was called "An Account of Some Skulls Discovered at Brandon, Suffolk". This involvement with anthropology was shortly to become consolidated, in grand style, on the Torres Straits expedition.

But important events had occurred in Myers' life - which must not be overlooked. He had entered Cambridge with the express intention of studying for medicine. Of his mood when leaving four years later he was to write "I had no idea as to my future career. Certainly I felt disinclined to medical practice. My main interests were by then divided between anthropology and experimental psychology".

In 1895 Myers left Cambridge. His life long interests and beliefs were now more or less established. Anthropology, physiology, psychology were now his subjects. His heroes were Rivers, Haddon and Foster. He had studied Ward and Lotze as well as Wundt's psychology. But it was still to be another seven years before he took his first post in psychology.

But as well as providing formative heroes and intellectual influences Cambridge offered another important opportunity to Myers. This was to sharpen up his organisational talents. As a schoolboy he had built up an informal society for amateur theatricals. But when Myers arrived at Cambridge the fortunes of the University Musical Club were at a distinctly low ebb, both financially and artistically. He soon put a stop to this state of

affairs. "At Cambridge", he later wrote, "much of my undergraduate leisure time was spent in reorganising the financially moribund Cambridge University Musical Club". He also played a prominent part as a violinist. In fact he played in a chamber music quartet with Hugh Allen, later to become professor of music at Oxford, conductor of the London Bach Choir and director of the Royal College of Music.

D. Seven Years Away From Cambridge 1895 - 1902

Myers spent the next seven years in a remarkable number of places doing a remarkable number of things.

The period started in fairly spectacular style. Myers later wrote,

> "Soon after I left Cambridge in 1895 Dr. Haddon began to plan his Cambridge Anthropological Expedition to Torres Straits (New Guinea) and Sarawak (Borneo), and asked me if I would join it. He proposed that it should leave England in March 1898 and I realised that by that time I could have just managed to obtain a medical degree at Cambridge, and at the College of Physicians and Surgeons."

The next three years of Myers' life were thus accounted for. He duly spent the first two qualifying as a medical doctor. This he did despite his growing realisation that his future probably lay elsewhere. His leisure time in these two years, which he spent attached to his old haunt of St. Bartholomew's Hospital in London, was largely spent in musical activity. In 1898 he successfully qualified as a Physician obtaining the degrees of M.B. and B.Chir.

Full details of Haddon's Torres Straits expedition are given in the following chapters. Here we attempt to concentrate on the part this episode played in the life of Myers. Apparently Haddon was extremely unmusical. Quiggin (1942) writes, "During the 1888-9 visit to the islands he had been conscious of his own limitations, especially in two spheres, music and languages. He could describe dances, but was deaf to the accompanying tunes". Thus Myers became Haddon's natural choice for he was one of Rivers' best psychologists, he was musically gifted and moreover he had a developing interest in anthropology.

On Myers' side his love of travel, his ready acquisition of foreign languages and his twin desires to pursue anthropology and psychology ensured his "immediate acceptance".

Myers was apportioned, in particular, the work on rhythm and music, hearing in general, taste, smell and reaction time. So much data was eventually collected that Myers spent a great deal of his time before the war preparing no less than 13 published papers deriving directly from his Torres Straits studies.

For Myers, the expedition was an introduction par excellence to the application of the methods of experimental psychology. Quiggin's book is a testament to the industry of the expedition - as is the remarkable number of publications deriving from it. It is hard indeed to imagine a more thorough and varied grounding in the psychological techniques of the day. (Myers' own notes on the expedition still exist in the form of a hard-backed

book, which forms part of the Cambridge University Library's "Haddon Collection". It runs to some 193 extremely closely packed - and almost illegible - hand-written pages. The notes, in so far as they can be read, take the form of a rather prosaic 'ship's log' with very little - unfortunately - of either personal or psychological interest apparently in them.)

When Myers returned, in April 1899, he must have come down to earth with rather a bump. Cambridge's treatment of Haddon was still near to insulting. There were no openings for Myers, it seemed, even in anthropology or physiology - let alone in psychology.

Thus in 1899 he took up a year's appointment as House Physician at St. Bartholomew's in London. During the year there occurred an event which finally settled his future. Myers wrote of this (1936),

> "As I have said, I had already decided not to become a medical practitioner, but largely through my intimate friendship with the late Professor A. A. Kanthuck, began at St. Bartholomew's Hospital, I was becoming strongly drawn to research in pathology. Had it not been for his premature and grievous death in 1899, I might have returned to a study of the subject. But his death and the experiences of the expedition left me in little doubt as to my future career."

Myers confirms this picture, in 1942, where he relates that now he came even more strongly under the influence of Rivers and Haddon. Their personalities, he claims, were instrumental in his final drift towards psychology and the social aspects of anthropology.

But this absence from Cambridge has still a sting in the tail. Almost at the end of his year at St. Bartholomew's, Myers became ill. Characteristically he took full

advantage of even this situation. His health forced him to seek warmer climes. Thus he spent the rest of 1900 and the Winter of 1901-2 in Egypt.

He continued to work very hard and by the end of 1902 he had progressed along a number of fronts. He wrote and published his M.D. Thesis on Myasthenia Gravis. This was his final, purely medical work. He continued to write up his Torres Straits data publishing studies on Papuan hearing, visual acuity among the natives of Sarawak and "Malay Midwifery". He wrote a reply to Ward's Giff-ord Lectures "Naturalism and Agnostisism" and made an experimental study "On the Pitch of Galton Whistles". He began extensive work on ancient Egypt. He studied hieroglyphics and managed to inveigle his way into some archaeological digs to study antiquities. Not content with this, he also studied native tatooing and carried out an extensive piece of anthropometrical work in Cairo and Khartoum. This involved studies among Egyptians and Sudanese soldiers which were later published as a series of five papers in the Journal of the Royal Anthropological Institute between 1903 and 1908.

The Spring of 1902 saw Myers, finally, return to Cambridge. His health was restored and he was finished with medicine. The seven years between 1895 and 1902 had seen Myers publish work in medicine, anthropology, psychology and philosophy. He was also a fully qualif-ied doctor. It must have been hard for him to give up the prospect of a secure career in medicine. But luckily that is precisely what he did next.

Perhaps the most notable single characteristic of Myers' psychology derives from this period. This is of course his eclecticism. Clearly he even found it difficult to commit himself to any single discipline. This seven year period saw him travel from St. Bartholomew's to Sarawak, from philosophy, to cross-cultural psychophysics and from Egyptian anthropometry to Galton whistles. These experiences clearly left their mark as his later vehement attacks on esoteric schools of all kinds was to show.

E. Return to Cambridge 1902 - 1909

In 1902 the Cambridge Laboratory reached its third home. Rivers' courses in the physiology of the senses were proving popular enough to justify expansion. Thus he transferred his small menage into the slightly more salubrious facilities of St. Tibbs Row.

This move prompted Rivers to ask Myers to return to Cambridge and become his assistant. Myers, was soon made responsible for a course on the "Psychology and Physiology of Hearing" while he assisted Rivers with three on the "Physiology of the Sense Organs".

Thus Rivers engineered Myers' return to Cambridge and secured his first academic employment. Myers was of course well versed in the physiological component of the course as well as being trained par excellence in psychophysical methods. On Myers' side the offer was fairly attractive - offering him an entrance to an academic career in his favourite subject. However, his only pay was out of "fees received" and the future growth of psychology was far from certain!

At first Myers had time to continue the compilation of his data from Egypt and the Torres Straits. He also soon began work on his Textbook of Experimental Psychology which was eventually published in 1909.

1903 brought along the move to Mill Lane and increased financial support from the University. The partnership of Rivers and Myers was clearly being most effective in attracting pupils and thus consolidating the position of psychology. In 1904 Myers received his first, though still unpaid, University appointment as "Demonstrator in Experimental Psychology". In this same year Myers became the second secretary of the B.P.S. (replacing A. F. Shand) and with Rivers and Ward established the British Journal of Psychology. At Cambridge he was rapidly becoming the leading force in the subject - not so much through academic argument and publication but through sheer political activity enthusiasm and organisation. For at this time Rivers was becoming further committed to his anthropological studies, while James Ward was veering towards philosophy. This left Myers very much responsible for the growth of experimental psychology - which was probably just as well as he had the organisational and social skills which the other two rather lacked.

But Myers was not content to restrict himself to furthering psychology at Cambridge. In fact in 1903 he very nearly left Cambridge altogether. Charles Sherrington, then Professor at Liverpool, had considerable sympathies towards psychology. He asked Myers to become a lecturer at Liverpool to promote psychology there.

Myers seriously considered the offer (he had after all no security of tenure or even guaranteed salary at Cambridge). In the end he refused because Sherrington could offer him no assurance that he himself would remain at Liverpool for the foreseeable future. But Myers was also involved at King's College, London. He taught psychology there, part-time, from 1904. In 1906 he was awarded the unusual post of part-time professor. One of his first and ablest pupils from this period was T.H. Pear. Pear became a very close and life-long friend of Myers. Pear was also, of course, Professor of Psychology at Manchester from 1919 - 1951.

Myers continued to commute to London to fulfill his duties at the B.P.S., until 1911, and at King's College until 1909. At King's he began his seminal work (with H. A. Wilson - later Professor at the Rice Institute Texas) on binaural phases differences in audition.

Psychology was now beginning to 'snowball' a little. Myers was firmly at the centre of events and in 1909 was so over-worked that he was ordered six months' rest and obliged to resign his Professorship at King's.

Rivers and Ward had both been developing new interests outside psychology. In 1906 this had led to a great increase in Myers' teaching load. In 1908 Myers was the prime force behind the instigation of the final campaign for the Psychological Laboratory. In this same year he was also polishing up the final draft of his Textbook for publication in 1909. So it is perhaps hardly surprising that his health should finally suffer.

But consolation came in 1907, when Myers became the first Cambridge "Lecturer in Experimental Psychology", - and his work load duly increased!

Such were the events leading up to 1909. The year is somewhat of a landmark for it saw all Myers' hard work place him in effective control of Cambridge psychology. He had become the main publicist and politician as well as the most productive - having eighteen publications in the 1902-1909 period. In 1908 he completed, in conjunction with Haddon, his immediate work on the Torres Straits data. From then his publications concentrated on experimental psychology with his first papers on 'Colour Vision' and 'Localisation of Sounds'.

In January 1910 Myers, along with Rivers and Ward, was appointed to a sub-syndicate of the full Psychological Laboratory Syndicate, to make the practical arrangements for the new laboratory. Inevitably Myers was left to shoulder most of this burden. Ward was 67 years old by this time and in any case was not so enamoured of "modern" experimental psychology. Rivers was also avoiding practical involvement.

In fact the size of Myers' contribution to the new laboratory is hard to overstate. The syndicate was only established in the first instance after Myers had disclosed that he had started an appeal fund with £3,000 from an "anonymous donor", who was, in fact, himself.

A note in the laboratory archives records in Myers' own handwriting the source of the other donations. They read, E. M. Smith £20, Rivers £10, Dawes Hicks £20, I. Seligman (probably this means Myers' father-in-law) £100. Sons of Wolf Myers £150, Mrs. Wolf Myers (Myers' mother) £250, C. G. Seligman £25, "Anonymous" £3,000. How right

indeed was Bartlett, in 1969, when he wrote "The Cambridge experimental laboratory was due almost entirely to him". (In fact the exact amount donated by Myers and his relations has never before appeared in print. All Myers owned up to (1936,1942) was "a considerable sum").

In May 1912 the post of Director of the Psychological Laboratory was created for Myers. He was confirmed in this (unpaid) position in October.

It might reasonably be thought that Myers had enough to do at Cambridge during this time. For he was not only involved with the laboratory he was also (very much in the tradition of Sidgwick) "endeavouring to improve the university status of the subject and to promote a wider interest in it". This effort took two forms. Firstly in 1911, he pressed for the acceptance of Psychology as a special subject in the ordinary BA degree. Secondly he was, in his own words, "largely concerned in establishing in 1912 the University Diploma in Psychological Medicine".

Also in 1911 Myers and Rivers became joint editors of the British Journal of Psychology. Myers took over sole responsibility in 1913, a post he held until 1924.

He also found time to pursue some research. Prior to the outbreak of war he was studying primitive music, synaesthesia, auditory localisation, attitudes to musical sounds and visual contrast. But his first love was the laboratory. Almost as soon as it was open Myers began to complain of the need for an assistant and also that the new laboratory was becoming over-crowded! Even before the war Myers managed to introduce to psychology, Bartlett,

Burt[1], Farmer, Hartridge, Mace, [illegible] Sprott, Thouless and C. W. Valentine.

One of his last acts before the war was the typical one of initiating a Tripos in Anthropology in 1914 - of which psychology naturally formed a part!

These pre-war years seem to provide an exceptional example of the intervention of one man in the origins of a new discipline. If Myers had been lured elsewhere almost certainly the laboratory would not have been built so soon.

That would have meant the loss, to other subjects, of a whole generation of psychologists at Cambridge. Clearly it is impossible to find fault with the General Board of Studies when they reported in 1912 that, "Here is an example of a new subject that had been built up in Cambridge largely owing to the energy of a single man".

G. Myers and the 1914 - 1918 War

Cambridge psychology made two main contributions to the war and Myers was behind both of them.

The Great War, Myers later wrote (1942) marked the beginnings of his shift from pure to applied psychology. Of course, it might be argued that for a man such as Myers this change was inevitable. However, the War certainly proved a most effective catalyst.

At the outbreak of the War he was involved in the study of primitive music. This time he was investigating some records of Australian music. (In point of fact Myers had a unique collection of early phonographic recordings of primitive music from all over the world. It was

[1] Burt had, in fact, already studied psychology at Oxford. Myers however found him his first academic employment - as Demonstrator at Cambridge.

a great source of pride and interest to him). Myers very soon became aware of the incongruity of studying primitive Australian music while there was a War going on. He wrote (1940 p. 2)

> "I tried in vain to concentrate my attention on it; but the feeling of its incongruity and unimportance increased to such an extent that by the end of August I could work at it no longer".

Thus Myers applied to go on active service to France. However, he was told that "no medically qualified volunteer who was over 40 years old could be accepted". However, Myers having made up his mind that a psychologist could be of use in the War effort was unlikely to be deterred by a small thing like this.

After a few weeks Myers travelled to France incognito. There he "succeeded in persuading the Commandant of the Duchess of Westminster's Hospital, which was about to open at Le Touquet," to appoint him as "Hospital Registrar". Early in 1915 he was commissioned into the Royal Army Medical Corps and was "instructed to proceed to Boulogne and to supervise the treatment of functional nervous and mental disorders occurring in the British Expeditionary Force". Myers was given the temporary rank of Major and was entitled "Specialist in Nerve Shock". He was rather perplexed at the nature of the other staff, writing (Myers 1940 p.16)

> "In the Royal Army Medical Corps there were Officers with special knowledge of pathology etc.....but I never met with a regular Officer who had any specialist's training and experience in mental or nervous diseases and disorders".

In 1916 Myers was posted to General Headquarters at St. Omer with the rank of Lieutenant-Colonel, and the new title of "Consultant Psychologist to the British Armies in France". Thus he became one of the very first military psychologists. Here he became well placed to study a wide range of shell shock cases, for he visited clearing stations near the front as well as permanent hospitals in more withdrawn positions. For Myers the time was one of "frustration and misery" (Rodger 1971). He was determined to take a humanitarian approach, to stress the psychological factors involved and to utilise all his knowledge of psychology and psychoanalysis in handling disturbed soldiers. Not surprisingly he was called as a witness at many courts martial . He met staunch opposition to his liberal attitudes and afterwards he considered that not much had changed the views of the Army authorities that "the shell shocked soldier was necessarily a coward and that a deserter must be either a certifiable lunatic or a criminal deserving only of being shot".

In fact although Myers' work was, medically at least, both successful and original it received niggardly recognition from the military. Seeking more favourable climes, Myers sought a position back in England. This he achieved in 1917 as an assistant to Dr. (Colonel) W. Aldren Turner at the War Office. His duties now were to inspect the military shell shock hospitals in England and Scotland and to attempt to bring a little harmony into their diverse procedures. He spent the first part of this tour of duty

at Maghull shell shock hospital in Lancashire - a period of his life which was to prove something of a turning point.

The hospital was run by a Dr. R. G. Rows. He had studied Janet and Freud and was quite prepared to countenance "mentalistic" theories and treatments of his patients. Being away from the front line also, no doubt, made this approach somewhat easier. Among the staff at Maghull, at various times, were Myers' old Torres Straits companions Rivers, McDougall and Seligman. There were also William Brown, T. H. Pear, Bernard Hart and Elliot Smith.

The second string of Cambridge involvement in the War came in 1917. It concerned the hydrophone work of HMS Crystal Palace which stemmed from Myers' 1908 paper on the 'Localisation of Sound'. An account of this project is given below in Chapter 10. This episode is important for Myers himself because it showed him how useful experimental psychology could be. The hydrophone work illustrated the need for efficient selection and aptitude tests and the need to consult psychologists in equipment design. It also demonstrated the usefulness of psychological techniques and the importance of equipment such as Myers' sound-proof room at Cambridge (described in Chapter 9).

The war changed Myers' outlook considerably. As he put it himself,

> "On demobilization I returned to Cambridge, fired with the desire to apply psychology to medicine, industry and education and becoming increasingly disgusted, after my very practical experience during the War, with the old academic atmosphere of conservatism and opposition to psychology".

Myers' work on HMS Crystal Palace and on shell-shock had left their mark. But there were also his discussions with that unusually talented and varied group of doctors at Maghull. It was there that his old friend and pupil T.H. Pear talked to him about industrial psychology and happened to mention the book written by Bernard Muscio, who was the demonstrator at the Cambridge Laboratory before the outbreak of war. Myers' swift conversion to industrial psychology is recorded not only in his own words, above, but also in his 1918 publication of 'Present Day Applications of Psychology'. As it turned out this pamphlet formed somewhat of a manifesto for the N.I.I.P.

Two honours were bestowed on Myers during the War. In 1915 he was elected as a Fellow of the Royal Society. Sherrington, who as we have seen earlier, had a high opinion of Myers, communicated his 1908 paper on "Localisation of Sounds" to the Society which duly secured his election.

The second honour was the C.B.E. Rather ironically, this was received for his medical services in R.A.M.C. In fact, after the armistice, Myers took the trouble to complain to the War Office about what he viewed as their "studied neglect" of the temporary medical officers who did so much good therapeutic work in the "shell-shock" hospitals. The only outcome was the award to himself. At first he refused it and only finally accepted when summoned to an investiture by the King.

H. Post War Cambridge

Myers' return to Cambridge precipitated what his great friend Pear (1947c) referred to as "the greatest ...

crisis of his life". His new enthusiasm for applying psychology, his distaste for academia and his twin desires to organise and initiate led him, before the end of 1918, to take a year's leave from Cambridge to reflect on his future. His decision, at the end of this year, was to leave Cambridge permanently and to found the National Institute of Industrial Psychology.

Psychology at Cambridge progressed steadily, if unspectacularly, after the War. Myers was re-instated as Director of the Laboratory and was elected as a Fellow of Gonville and Caius College in 1919. Two years later a Readership in Experimental Psychology was created for Myers, ad hominem. The annuity was a considerable £650. The Diploma in Psychological Medicine continued. In 1920 a special independent "Board of Psychological Studies" was established owing to the popularity of the courses organised by Myers and his assistants Bartlett, Muscio and Lawson. It seems paradoxical indeed that Myers should want to leave such a promising situation - especially as he had done so very much to create it.

But Myers himself saw it all in a very different light. In 1936 he wrote of this return to Cambridge thus,

> "I found that the wild rise of psychoanalysis had estranged the Regius Professor of Physic; I received little encouragement from the Professor of Physiology; and the Professor of Mental Philosophy, to my surprise, publically opposed the suggested exclusion of the word "experimental" in the title, now about to be conferred on me by the University, of Reader in Experimental Psychology. Thus medicine, physiology, and philosophy had little use then at Cambridge for the experimental psychologist".

It seems wrong to dwell too long on the negative reasons for Myers leaving Cambridge. For he thrived on positive activity, organisation, initiation and people. The secure life of a don, Fellow and possibly a Professor would not have entirely suited him - as indeed his good friend Pear confirms (1947c).

Before the end of 1918 he wrote and delivered two lectures to the Royal Institution under the title "Present-Day Applications of Psychology". It was these that led to his fortuitous meeting with H. J. Welch in October of that year. Welch was a business man, in fact a director of the importing firm Harrisons and Crossfield Limited. He had ideas of his own on personnel problems in industry and on the founding of some sort of Industrial Psychology Institute. Welch and Myers soon formed a firm working relationship, dreamed up the idea of the N.I.I.P. and then went to work on bringing it about.

During his year's absence from Cambridge Myers wrote his first book on Industrial Psychology. Entitled Mind and Work it appeared in 1921. By the end of 1919 financial support for the N.I.I.P. had been engaged by Welch and Myers. But before he launched himself fully in the direction of the N.I.I.P. he returned to Cambridge to secure the future of psychology there. He did this by ensuring that his Readership would not expire when he left and further that it would be passed on to Bartlett.

I. At the N.I.I.P. 1922 - 1939

Thus Myers gave up his highly successful academic career - a career which had brought him a secure, lucrative

Cambridge Readership and a much coveted F.R.S. From this point on his life was spent almost entirely with his brain-child the N.I.I.P.

The Myers family had to move to London. It was with some reluctance that Mrs. Myers left Cambridge. However, it seems that Myers himself was more than pleased to return to his native town. They spent most of this period in an imposing house in York Gate, near Regents Park.

The full story of the N.I.I.P. is told in Chapter 10. From Myers' point of view it had become his full-time occupation in 1920. Then he had become Director and George Miles, his assistant. These two were the only full-time employees. By 1931, when Myers became Principal, the Institute had over 50 full-time staff.

A good account of Myers' early days with the N.I.I.P. is given by Burt (1946). From 1920 onwards, he writes,

> "The Institute was above all else his chief concern. The early struggles required to establish it on a firm financial and scientific footing, and its subsequent development and successes, Myers and Welch have themselves recounted in their book on Ten Years of Industrial Psychology. Once again as the first head of the Vocational Guidance Department of the Institute, I had the privilege of working under him; and from our talks at the close of many a tiring day, when we were in the habit of comparing work in London with work in Cambridge, I realised what a tremendous sacrifice he had made in giving up a quiet life devoted to scientific research (which had always been his chief ambition), and plunging into the arduous task of organisation, administration, making contacts with private individuals, public bodies, and commercial firms, securing financial support, and seeking to introduce scientific ideals and methods into the world of commerce and manufacture. At times, especially in the earlier stages, he suffered a not unnatural

> depression, which the recurrent and inevitable setbacks seemed at first to justify, and which his own inherent modesty often tended to enhance. Nevertheless, the great and growing success of his work, and the somewhat tardy recognition of his labours from high quarters, both in scientific circles and in the world beyond, brought him a growing and, I think, a final satisfaction.
>
> During the first few years of the Institute's life, he was personally concerned with every phase of its activities. He supervised the small staff of investigators; he visited factories up and down the country; and acquainted himself at first hand with every phase of industry. When, for example, the Institute undertook an investigation of the gold mines of South Africa, he went out to see the mines at first hand and to discover by direct observation the conditions under which gold was obtained.
>
> Frequent contacts with industrialists gave him opportunities for expounding in personal conversation the ideas and ideals that he had expressed in his writings and lectures; and brought him an ever-widening group of acquaintances and friends to whom he turned when seeking funds for expanding the Institute's activities or extending its investigations. Much of the support the Institute received was the direct result of his capacity for communicating to others his own enthusiastic belief in the great part industrial psychology has still to play in the life of the nation."

The combination of Miles and Myers at the Institute's helm was an interesting one. Myers, as Burt suggests, soon became the N.I.I.P.'s great publicist. He was the N.I.I.P.'s public face; its after dinner speaker. It was Myers who spread the word amongst the country's industrialists. Grand schemes and sweeping ideas were Myers' forte. (It was a characteristic he shared somewhat with Bartlett). Miles was the man who attended to the details. (Rodger 1971, 1977)

As the N.I.I.P. grew so Myers was increasingly "pushed upstairs". In 1938 he became 'Honorary Scientific Adviser'. In this capacity he continued to oversee the contents of the Institute's Journal - of which he was still, nominally at least, Editor. He also continued to be active on the Institute's main committees. However, his full-time involvement more or less ended at this point.

What did Myers think about the progress made by his N.I.I.P.? In 1932 his tone is wholly optimistic. He writes about the growth of the staff of the N.I.I.P., the increased subscriptions and the "considerable body of research work" published by its investigators. By 1942 however, his tone was more considered and reflective. There he writes in a somewhat disenchanted manner. He was clearly a little depressed that so much of the N.I.I.P.'s. work had to remain unpublished - for much of it was private work carried out on contract. The constant seeking after money also clearly annoyed him.

But perhaps the greatest source of personal disappointment to him was the continuing friction between the N.I.I.P. and the government sponsored Industrial Health Research Board (I.H.R.B.). He writes that the I.H.R.B. seemed jealous of the N.I.I.P.'s. achievements and also of its standing in Whitehall. The N.I.I.P. was <u>forced</u> to indulge in publicity and contract-seeking - by its very nature as a privately financed Institute. But I.H.R.B. consistently failed to grasp this - at least claims Myers. As a consequence they became annoyed (Myers especially singles out Sir Walter Fletcher of I.H.R.B.) at what they saw as the

N.I.I.P.'s. constant blowing of its own trumpet and (even worse) encroachment on their territory. Clearly the atmosphere was not conducive to co-operation. "So much better work", writes Myers, "could have been carried out if jealousy had not substituted hostility for co-operation in the relations between the Institute and the Board. Sir Walter Fletcher used to envy and decry the Institute's adverts".

This state of affairs clearly caused some pain to Myers. So much so that as Rodger (1971 & 1977) points out his manuscript "Fifteen more years of Industrial Psychology" was almost embarrassingly full of bile towards I.H.R.B. Indeed, after Myers' death, Rodger himself, after consulting Bartlett, decided the book was so full of these private grievances that he should not revise and add to the manuscript as Myers had requested. Thus the book remained unpublished. (The manuscript remains in Rodger's possession).

However, this apart, N.I.I.P. was undoubtedly a success. Myers had every right to feel proud of putting Britain on the map in Industrial Psychology.

But Myers had not restricted himself completely to N.I.I.P. He still kept firmly in touch with academic psychology.

Until 1924 he was still Editor of the British Journal of Psychology, when he handed the role over to Bartlett. For all this pre-war period he also paid frequent visits to Cambridge where he would invariably look in at the laboratory. The connections between N.I.I.P. and Cambridge were strengthened during these years by the constant flow

of pupils from Bartlett's tutelage to the London offices of the Institute. Farmer, Chambers, Rodger, Muscio, Oldfield and Knight were among the earliest. In 1923 Myers organised the Seventh International Congress at Oxford. This he did in his capacity as the first President of the British Psychology Society, a post which he held from 1920 - 1923. In fact in 1919 he had been a leading force, as one might have expected, in the re-organisation of the B.P.S., (for details see Edgell 1947). The B.P.S. had faced something of a financial crisis and had asked Myers to draw up a rescue plan. His solution was to radically re-structure the society. Indeed it was Myers who organised the B.P.S. into General, Medical, Educational and Industrial sections. Each section supported its own journal - a system which has survived, if a little reformed, to the present day.

Naturally his pre-occupation with N.I.I.P. severely limited Myers' opportunity for research from 1920 onwards. However, his bibliography from this time is considerable both in volume and variety. In all he published no less than 77 items in the last 20 years of his life. Naturally the industrial items predominate. His papers on general issues in psychology also figure largely - perhaps because, with his reputation as a founding father, he was asked to give many invitation lectures. His medical work revived with the onset of war which provoked the publication of his "Shell Shock in France 1914/18" but it had also continued as an aspect of his industrial studies. Rather surprisingly his last publication in the musical field was in 1933.

J. Myers' Role in World War II

The outbreak of World War II found Myers at the age of 66, more or less in retirement. In the previous year he had ended his day-to-day employment at N.I.I.P. He and his wife had removed themselves to their country home at Winsford, near Minehead, Somerset.

The War however was to make Myers' last years rather more adventurous than he might have expected.

One noticeable development in the second Great War was the willingness of the Armed Forces to seek and use the knowledge of psychologists. All three forces recruited heavily from the ranks of psychologists for both selection and training purposes. To Myers' obvious delight (1942) employees of the N.I.I.P. were eagerly sought after. Myers himself was called to serve on the Adjutant General's committee which supervised the work of the Directorate of the Selection of Personnel (D.S.P.) for the Army.

It must have been satisfying indeed for Myers to see not only the methods, techniques and expertise of the N.I.I.P., but also its personnel, contributing so much to the war effort. As Bartlett (1947) writes of this time, it was in the Second World War that "many of the things for which he had vainly striven in the first were established as a matter of course".

Myers died on October 12th 1946 at his home in Winsford. He was 73 years old but he had been busily involved with Psychology even in the last few days of his life. His wife and five children, Dorothy, Edmund, John, Joan and Ann survived him.

Myers was of fairly robust constitution. This together with his great energy and activity caused some surprise at his death even at the age of 73. Bartlett in a letter to Drever (Senior), sometime afterwards writes,

> "Nobody thought of Myers as being likely to succumb very quickly to illness, not even the members of his own family. I understand that he was at a public luncheon of the Institute only a few days before he died and seemed to be in unusually good form. He went back home and retired to bed with what was thought to be a cold, developed pneumonia, and then his heart gave out only a few hours later. The news came as a shock to most of us, but no doubt it was better than for him to have a long drawn out period of immobility".

K. Conclusions

Thus Myers' long extraordinarily varied and innovative life came to a close. When he died three of the U.K.'s leading psychological bodies were greatly indebted to Charles Myers. There is indeed much truth in the words of Bartlett "he built a Laboratory, a Society and an Institute". But what was he like this engineer of so much of British psychology?

According to those that recall him, Myers had a gentle rather charming and somewhat aristocratic manner. He was extremely sociable - as Pear has put it he "loved the warm smell of humanity". Bartlett has gone so far as to suggest that it was his love of people that led and held him to psychology. He was quietly spoken. In writing and speech he was extremely considerate and cautious. As an editor of the British Journal of Psychology and the various N.I.I.P. journals he was extremely fastidious "not to say pernickety" (Bartlett 1965). In

conversation his famous "silences" became almost legendary. These disconcerting events, his contemporaries seem agreed, stemmed from his careful, considered academic approach. However, his rather hesitating manner made Myers a rather "variable" and "deliberate" lecturer - one who preferred accuracy and thoroughness to fluency.

The other aspects of Myers' character can be best approached through events in his life. At the start of his career it seemed as if he might fit into that strange category of the wealthy Victorian 'amateur academic' of which Francis Galton was perhaps the best example. After all, Myers was wealthy; he studied natural science, became qualified as a doctor, studied psychophysics , physiology and psychology, he served as a member of an anthropological expedition and made an original study of Egyptian hieroglyphics - all before he was 28.

But by 1902, at the age of 29, he had firmly set his course towards psychology. Twice in his life Myers gave up a secure, well paid and challenging position. In 1902 it was to become Rivers' unofficial and unsalaried assistant. In 1922 it was to run the N.I.I.P. There was clearly something in him which drew him towards the early days of new enterprises.

A few last words might be usefully added here about Myers' Jewish business background. Apart from providing him with the wherewithal for a first rate education this setting provided Myers with invaluable contacts in later life. The Myers' family were close to several prominent London families including those of Sebag-Montefiore (of

stock broking fame), Waley-Cohen (who produced a Lord Mayor of London during the period) the Salamans, the Samuels and most importantly the Seligmans. Myers married Edith Babette Seligman in 1904. Her brother Charles (who was not incidentally the same C.G. Seligman F.R.S. who came to the Torres Straits) was an eminent merchant banker. With such family connections one can see that Myers provided in himself a most valuable father figure for the N.I.I.P. which of course depended almost entirely on the good-will of the various business concerns which employed its service.

CHAPTER 9

THE INSTITUTIONALISATION OF PSYCHOLOGY AT CAMBRIDGE 1897-1922

A. The Cambridge Laboratories 1897 - 1902

The first purpose built psychological laboratory in the U.K. was opened in Cambridge in 1912. This step was a great advance, and psychology now enjoyed vastly improved facilities. The 1912 building has remained the home of the Cambridge laboratory in Downing Street ever since. But in fact the Downing Street laboratory was the fifth home of experimental psychology in Cambridge.

The first home, as we saw in Part 2, was with James Ward in 1891. The second home was in a single room in the physiological laboratory under the direction of Rivers after 1893. Rivers, it may be recalled, was appointed officially by the Board for Biology and Geology, in 1897. The Professor of Physiology on this Board was Foster and so it is not surprising to find Rivers with the use of these facilities. It was in this single room that Rivers conducted his experiments and some new practical courses. With these he was now able to supplement his two lecture courses in "Experimental Psychology" and his one on "The Physiology of the sense organs".

Bartlett writes of this period (1937(1)p 102),"..... a few students worked hard at the special senses, especially at vision, at various elaborations of psychophysical method - for all his life Rivers retained a profound belief in the value of these as a mode of training - and did some study of fatigue and drugs. In this single room a

student who was to play by far the greatest part in the firm establishment of psychology at Cambridge - C.S. Myers - began his life work". This small room, the second home of Cambridge experimental psychology, was important in two ways. Firstly, it gave Myers his introduction to practical methods. Secondly, in this room Rivers began his studies on fatigue and drugs. This was the start, in Cambridge, of the application of psychological experiment to real-life situations, and marks a sharp contrast to the rather "purer" psychophysics and psychology of Ward and much of the contemporaneous German work.

This single room was soon outgrown, for Rivers' courses were both popular and successful. With the support of the Boards for Moral Science and Biology and Geology he applied for better facilities. The Boards "called the attention of the Senate to the need for more adequate accommodation for the teaching of Experimental Psychology". The Senate agreed to the proposal that £35 a year should be allocated "in consideration of the assignment of certain rooms in a building in St. Tibbs Row as a Laboratory for Experimental Psychology".

The work continued much as before, but psychology's stay in this, its third home, was to prove short lived. But before we consider this next move it is time to reflect on the trends in lecturing occurring at this time.

B. The Growth of Lectures 1897 - 1913

Until 1897 the principal official lecturers in psychology had been Ward and his pupil and follower G.F.Stout.

Despite Ward's lectures and demonstrations in psychophysics the psychology taught reflected the philosophically inclined psychology of these two men. Indeed, as we have noted, psychophysics for Ward and Stout was merely a useful adjunct just as were the physiological studies of the senses.

In 1893 Rivers arrived and the Board for Biology and Geology begins to take a firmer hand in psychology's history. The flavour of Cambridge psychology began to change quite distinctly. Rivers led psychology steadily away from its place as a sub-branch of philosophy which could be aided by psychophysics. He led it towards a place among the biological sciences, as a discipline which stood on its own, and was basically observational. Under Rivers, psychology began, for the first time in Cambridge, to have more to do with the laboratory than the armchair.

All this time Ward had continued to give his lectures. He gave three or four courses in psychology each year, usually including psychophysics, throughout the 1890's. We have also noted that Rivers, in 1897, began giving two courses in experimental psychology and in sensory physiology, with practical courses attached. In 1901 Ward's next protege W. E. Johnson began to lecture and from this point, until the end of the period under consideration, these two shared the Moral Science Board's load of about five courses a year. But the more important growth began in 1902 with the move, just described, to Tibbs Row. These new facilities enabled Rivers to continue as before, but

now with the addition of no less than four new practical courses. Three of these were run by Rivers and Myers together on "The Physiology of the Senses" and one by Myers alone on "The Psychology and Physiology of Hearing".

This pattern of a heavy practical programme run by Rivers and Myers supplemented with more theoretical contributions from Ward and Johnson continued until 1910 when the examinations were re-organised. In recognition of his heavy timetable Myers was appointed a University Demonstrator in Experimental Psychology in 1904. In 1906 he began a lecture course entitled "Animal Psychology". Myers was thus the holder of the first Cambridge post purely in Experimental Psychology.

The period 1898 - 1914 was of course one which was marked by Rivers' frequent absences from Cambridge - as we have seen in Chapter 7 Myers therefore was left with far more responsibility and hard work than the official records indicate.

Also, in 1904, Rivers, Myers and Ward together founded the British Journal of Psychology. A glance through the first volumes of this journal will reveal that this marked an important step. The only contemporaneous psychological journal was Mind. This had, of course, been founded by Alexander Bain in 1876 and still reflected a huge philosophical bias which was rapidly becoming outmoded.

The new British Journal, it is true, did contain several theoretical contributions. But many of them are recognisable as theoretical psychology rather than philosophy. More important is the fact that the journal was

sorely needed as a forum in which to publish the large amount of experimental work which was emanating from the Cambridge Laboratory. As Bartlett (op.cit.p. 103) puts it "A glance through some of the earlier volumes ..... will show what a lot of work, and what good work was being done there".

But now we must return to the main theme. The amount of work undertaken by Rivers and Myers grew steadily in the years 1902 - 1906. On 15th May 1907 (see Vol. 38 The Reporter p. 1139) Rivers felt compelled to write to the Chairman of the Moral Sciences Board complaining of overwork. It will be recalled that Rivers was a lecturer appointed by the Board for Biology and Geology. He wrote here complaining of the amount of teaching he was forced to undertake for the Moral Science Tripos - and on the physiological side. "It will not be possible for me to carry out the work adequately", he wrote and added that Moral Sciences, in his opinion, should appoint a lecturer in Experimental Psychology. The General Board agreed with Rivers' suggestion. Thus he resigned part of his brief in "Physiological and Experimental Psychology". From October 1907 Rivers received a £100 stipend as lecturer in the "Physiology of the Senses" and Myers was appointed to the £50 per year post of lecturer in "Experimental Psychology". The first lectureship to be devoted entirely to psychology was thus created.

As we have seen, in Chapter 7, the real reason for Rivers' appeal to the General Board was his increasing

preoccupation with anthropology. By 1907 he had already completed his expedition to the Todas in Southern India. In 1908 he went off to Melanesia and thereafter was engaged almost solely in writing up his findings. Thus Myers' appointment as a lecturer effectively marks the beginnings of his time as Cambridge's leading psychologist.

Myers' old, unpaid, post of Demonstrator in Experimental Psychology was taken over by E. O. Lewis in October 1907. The lecture programme for 1910 - 1911 clearly illustrates the effect of Rivers' departure from the scene and the influence of the new capacity for practical work which Mill Lane and Myers' advocacy afforded.

Rivers and Myers gave a course each, with practical work, on the physiology of the senses. Myers gave two courses on advanced experimental psychology, with Ward he delivered two courses on introductory psychology and with Lewis a further two on elementary experimental psychology. Johnson and Dawes Hicks were also engaged to lecture on psychology. Meanwhile Ward gave two courses of his own "Advanced Psychology" and two on metaphysics.

Another important move occurred in 1910. This was the acceptance of psychology (after a considerable campaign led by Myers) as a subject for examination in the Cambridge ordinary B.A. degree. This secured for the subject a rather larger audience than that afforded by its previous place in the Moral Science Tripos. The syllabus was wide-ranging. Candidates had to sit exams

in six topics which were:-

1. The physiology of the nervous system and sense organs.
2. General - psychophysics and illusions.
3. Practical - elementary experiments on sensation and perception.
4. General - memory, hallucinations, pleasure and pain, emotion, personality, hypnotism, instinct and habit.
5. Practical - reaction time, organic aspects of feeling and emotion, association time, imagination.
6. Application to Education - infancy, individual differences, memory training and discipline.

   OR

   Animal Psychology - evolution and the nervous system and brain, habit, intelligence, play and social behaviour.

The last two topics were relatively new ventures. The first course on Animal Psychology was run by a certain E. M. Smith who was later to become Lady Bartlett. The substance of her courses appeared as <u>The Investigation of Mind in Animals</u> in 1915. In this book Smith reviewed the work of Watson, Yerkes, Thorndike, Pavlov, Hunter and Hobhouse. Smith herself carried out experiments on colour vision in dogs in 1910. Thus it was largely through her

efforts that Cambridge psychologists became aware of the early work on conditioning. Smith took a firmly experimental view of her subject. The problem of animal intelligence is not a philosophical one she held. Rather, she writes, it is one to be solved by "careful study and systematic observation of the behaviour of different organisms under widely varying conditions". She obviously admired the techniques of Pavlov most of all. These offered, she points out, new controlled methods to examine sensory discrimination as well as habit and learning. She was less impressed with Thorndike claiming that learning was a far more complicated process than he suggested. He could not, for example, account for imitation nor for the sudden intuitive one-trial learning so often exhibited by animals.

But perhaps the greatest importance of Smith's work lies elsewhere. For, it clearly shows that it was not out of a lack of knowledge that the gauntlet thrown down by Pavlov, Thorndike and Watson was not picked up by the early Cambridge psychologists. Bartlett, after all, was later married to this British pioneer of animal psychology.

The introduction of the course on the educational applications of psychology is also of interest. This marked the first attempt by Myers to introduce applied psychology to his courses. The first courses were given by a Mr. Fox who Myers borrowed from the Cambridge "Train-College for School Masters".

Dr. Mayo, as energetic as ever, did not miss the opportunity to grind his favourite axe when this new syllabus came to the Senate for approval. He said that he

"deprecated any concern whatever on the part of the University with Psychology", it was a subject which put the University "to great expense". Apart from this, he continued, "it is an attempt to explain all mental phenomena by merely physical causes. They were really a degrading of the mind to mere materialism".

Myers was also busy elsewhere. As we have seen, in Chapter 8, he was largely responsible for the institution of the Cambridge "Diploma in Psychological Medicine". On the 21st of February 1911 the Special Board for Medicine adopted the suggestion for the diploma which had apparently been put to them by Rivers and Myers. It was among the first of such courses in the country and was fairly comprehensive. Sir Clifford Allbutt speaking in the Senate called the diploma "a revolution in mental care". The syllabus included studies, many practical, of the physiology of the nervous system. It also covered neurology, the administration of "Lunacy Law and Asylums" as well as more general psychology.

Thus, under Myers' direction, psychology by 1913 had made substantial progress into the new fields of animal psychology, education and medicine.

One of Myers' last pieces of organisation before the War was to get established a Tripos in Anthropology in 1914. This of course included much psychology. In fact Quiggin (1942) has noted that Rivers and Myers were of considerable assistance to Haddon in his continuing struggle to establish anthropology at Cambridge.

However the next major step in this story is the building of the Cambridge Psychological Laboratory and to that we must now return.

C. The Laboratory Reaches Its Final Home 1902 - 1913

We left the laboratory in its third home, the "dismal rooms" as Bartlett calls them (op.cit.) in St. Tibbs Row. The next move was not that much of an improvement.

For the fourth home of the laboratory was at 16 Mill Lane. The University Press owned this building, but were not using it and so generously allowed the premises to be occupied by the psychologists in 1903. The Moral Science Board successfully appealed to the Senate for an increase to £50 (from £35) in the annual allowance for upkeep and equipment. Mill Lane was to be the longest stay so far.

From the accounts that are left it appears to have been a building with some considerable individuality. "It is a wonder that Behaviourism did not first grow there, instead of later and elsewhere" notes Bartlett (op.cit). "The river was nearby. Rats abounded. Anybody could observe startle reflexes firing off in all directions". The Moral Science Board (Reporter Vol. 40 p. 270) was later to call it "a damp, dark and ill-ventilated cottage". Even the Vice Chancellor of the University (Reporter Vol. 41 p. 593) referred to Mill Lane as "a tenement which is an actual disgrace".

However, the experiments progressed. The work on the special senses still had pride of place diversifying

by this time into work on reactions to sound and colour, optical illusions and the effects of fatigue on perceptual processes.

Fortunately photographs of the inside of Mill Lane have survived (See Appendix). The apparatus included a primitive tachistoscope, revolving discs (for colour mixing and illusions) an ergograph, a Stern Variator and an early Eddison Phonograph with recording facilities. In fact Mill Lane housed most of the apparatus used by Rivers and Myers for their published research of this time. Rivers investigated colour vision, illusions and the effect of drugs on muscular and mental fatigue. Myers began his research on audition.

But, as the photographs indicate, Mill Lane was certainly not over spacious. Rivers and Myers' courses continued to attract great interest and large numbers. This, together with the growing amount of research taking place there meant that psychology at Cambridge was rapidly outgrowing its fourth home. In December 1908 Myers began to lead the fight for a new purpose built psychological laboratory. The first move was the publication of the pamphlet reproduced here. Firmer political action was to follow.

The year 1909 was an important one for Cambridge psychology. A student with an external London degree, from Stow-on-the-Wold, named F. C. Bartlett first came to St. John's. On the 12th November (see Reporter Vol. 40 p. 270) the Moral Sciences Board took sides with Myers and decided to press urgently for a larger, and new,

PLATE 9

Myers enrolled the support of the Cambridge University Association to assist his fund raising activities for the new laboratory. This pamphlet, of December 1908, was the result. It marked the beginning of the final campaign for the laboratory.

Confidential

CAMBRIDGE UNIVERSITY ASSOCIATION

STATEMENT OF THE CASE FOR THE ESTABLISHMENT OF A LABORATORY FOR EXPERIMENTAL PSYCHOLOGY

December, 1908.

The growing importance of Psychological Sciences has been recognised for many years past in the Universities of Europe and America. Nearly all the principal Universities of Europe and the United States, for example, Leipzig, Berlin, Göttingen, Paris, Turin, Harvard, Yale, Columbia, Cornell, provide special buildings devoted to instruction and research in the subject.

In this country Laboratories and special Lectureships have been established in London, Edinburgh, Glasgow and Liverpool. An excellent Psychological Laboratory has recently been built at Oxford as an annexe to the Physiological Laboratory.

So long ago as 1877 Cambridge made known its want of a Laboratory for Experimental Psychology. Had that Appeal been successful the Cambridge Laboratory would have been the first to be established in the world.

The application of the Experimental Method to Psychology has given a very great impetus to the progress of the science. Experimental Psychology has now the most important practical bearings on modern social conditions. The following may be cited as typical lines of research :—

1. Sensations, normal and abnormal (e.g. colour blindness).
2. The conditions of memory and the methods of learning.
3. The play and range of attention.
4. Illusions of perception.
5. The conditions of fatigue and the effects of practice in mental and muscular work.
6. The effect of drugs (e.g. alcohol or tea) on mental and muscular efficiency.
7. The mental characters of normal and defective children, primitive peoples and animals.
8. The conditions of the aesthetic appreciation of form, colour, harmony, and rhythm.
9. Hypnotism, multiple personality, insanity and other abnormal mental states.

Since 1897 a Lectureship on the subject has existed at Cambridge, and practical instruction in the methods of Experimental Psychology has been given since that time—but in buildings totally unfitted for the purpose. At present the apparatus of the Laboratory is housed in a damp, dilapidated and insecure cottage in Mill Lane, the property of the Syndics of the University Press, who have most kindly permitted the University to make use of it until the ground be required for the extension of the Press buildings.

The time has now arrived when it is absolutely necessary for the University to provide proper accommodation for a Laboratory, if instruction and research in Experimental Psychology are to be efficiently carried on. Of late years the small rooms of the cottage have proved utterly inadequate to receive the increasing number of students. Indeed at times it has been necessary to divide the classes and to duplicate the demonstrations and practical instruction given. For the same reason, it is at present impossible to offer a course in the subject to those who are being trained as teachers, although such teaching is provided in many of the Universities of England and Scotland and of the United States.

It is estimated that at least £4000 would be required in order to build the Laboratory and class-rooms which are needed. A larger amount is very desirable in order to provide for the proper upkeep of the buildings and other expenses of the department. It is anticipated that the University will not find any difficulty in assigning a suitable site for the Laboratory. But owing to the present demands on its income the University will for a long time be incapable of defraying the cost of building.

psychological laboratory. In a letter signed by Keynes, Myers, Sorley, Johnson and Stout they wrote,

> "The Board desire to call to the attention of the Senate the urgent need of more adequate accommodation for the Laboratory of Experimental Psychology. At present the department is housed in a damp, dark and ill-ventilated cottage at 16 Mill Lane, the property of the Syndices of the University Press who are good enough to allow the department to occupy it".

The cottage, they continued, was

> "being used by 14 students, 2 advanced students and three graduates engaged in research as well as by a lecturer and demonstrator both engaged in their own research programmes".

The Board pointed out that, "The building is incapable of satisfactorily accommodating even half this number of students". They concluded their appeal by pointing to progress elsewhere notably in Oxford where they said a Cambridge man (presumably they meant McDougall) had been forced to go through lack of facilities in Cambridge.

The motion went for final approval by the Senate, early in 1910. It took the form of a proposal recommending the establishment of a syndicate which would then, for not more than £50, obtain estimates from an architect. Myers let it be known that the £3,000 promised to him by an "anonymous donor" would be withdrawn unless progress was rapid. The donor was of course himself. However even under this duress and for this limited proposal it was by no means plain sailing.

Psychology's old enemy Dr. Mayo voiced some interesting objections in the Council of the Senate (see the

Reporter Vol. 40. p. 361). Give me any member of the Moral Science Board for a quarter of an hour, he said, and he would convince them that their experiments were not really psychological. "On principle," he continued, "he objected very strongly indeed to the grant of one single penny to Experimental Psychology". The terms experimental and psychology "were contradictory terms" in any case, he claimed. It was left to a Mr. Shipley in the Council, to remind the Senate that in 1877 Ward and Venn had wished to set up a laboratory. The lead in psychology could have belonged to Cambridge, he pointed out, if it had not been for the conservatism of the Senate. Instead, it had passed to Wundt in Leipzig. He went on to call Mill Lane, an "antiquated, ill-ventilated and rat-ridden disgrace".

Mr. Shipley's defence of psychology proved persuasive, for on 20th January 1910 (Reporter Vol. 41 p. 561) with the Senate's approval a syndicate was formed. It had eight members including the Vice-Chancellor, (R. F. Scott) Ward, Rivers and Myers.

They went swiftly to work. At the first meeting a sub-syndicate was formed. Ward, Rivers and Myers were empowered to prepare a report on the nature and size of the rooms required, a photograph of which appears here. Soon architect's estimates of £3,630 were obtained for such a building, on the University's Downing Street site. There, a new laboratory for physiology was to be built, and the syndicate proposed that the new psychology laboratory (ibid p. 593) be built "to the south of and adjoining the proposed building for the Department of Physiology".

PLATE 10

As a result of Myers' campaign (see Plate 9) Ward, Rivers and Myers were appointed to a sub-syndicate to plan the new laboratory. These proposals were the result.

For the Psychological Laboratory Syndicate

SCHEME OF THE PROPOSED PSYCHOLOGICAL LABORATORY

Ground Floor (1320 square feet).

Cloakroom and lavatory	100	square	feet.
Lecturer's private room	200	„	„
Demonstrators' room	200	„	„
Library	320	„	„
Workshop	200	„	„
Animal room	300	„	„

First Floor (1320 square feet).

Large practical class room	540	„	„
Dark room	200	„	„
Two small practical class rooms, each 290 sq. feet	580	„	„

Second Floor (1320 square feet).

Five research rooms, each 200 sq. feet	1000	„	„
Sound-proof room	320	„	„

The total area is 1320 sq. feet. If the height of the building be assumed to be about 60 feet, and due allowance be made for a sloping roof and for foundations, the building will contain approximately 66,000 cubic feet. At the same time it is probable that a skilful architect would contrive to save a good deal of space by reducing the height of the smaller rooms and by throwing the second floor partly into the roof.

The above rooms may be regarded as a minimal requirement for the laboratory. Larger rooms and a greater number of research rooms, together with an additional private room, are desirable.

A basement appears to be unnecessary. If built, it may possibly prove useful to the Physiological Department.

It is assumed that a small lecture theatre, capable of accommodating fifty persons, can be provided in the main Physiology building and that the Demonstrators' room can open directly into this theatre.

The Workshop should communicate directly with the exterior.

The Animal room must be well lighted and should communicate directly with a grass plot.

The large practical Class room, if lighted on one side only, should be narrow and long.

The Dark room should communicate with the large practical Class room.

The rooms on the second floor must have floors of cement.

The walls of the sound-proof room must be enclosed within outer walls on all sides. They are to be made of layers of felt, porous stone, wood and cork composition, following the construction of Professor Zwaardemaker's room at Utrecht.

Ample provision must be made for electric-light plugs and for wiring to provide electric and telephonic communication between various rooms.

Water and gas must be laid on throughout the building.

J. W.
W. H. R. R.
C. S. M.

February 1910.

The Drapers' Company, who were largely funding the Physiology building, saw no objections to sharing a common staircase for joint access. The syndicate also reported that "It is clear that experimental psychology and the physiology of the nervous system and the sense organs are so nearly connected that the two subjects can only gain by bringing their laboratories into close relation with one another".

Progress was reasonably smooth from then on. Although of course, with Myers and his relations providing most of the money the Senate was not called upon to dig far into its coffers. Even so Dr. Mayo (ibid p. 660) felt obliged to say;

> "No candid member could fail to perceive that there was nothing called psychology in the scheme that could not be carried out in every detail by the Department of Physiology. The eyes and ears which make us cognisant of material things are available to physiological study".

Despite the valiant efforts of Dr. Mayo the foundation stone was laid in 1911. Work in experimental psychology began in the new facilities in 1912. On May 15th 1913 Myers was appointed as Director of the Psychological Laboratory, and the laboratory was officially opened. The General Board of Studies confirming this appointment paid fulsome tribute - as well they might - to Myers. They could grant him no additional stipend over and above his lectureship. But they noted that, "The building was largely endowed by his own family. Here is an example of a new subject that had been built up in Cambridge

largely owing to the energy of a single man". The final costs when the building was officially opened in 1913 was around £4,250 with £3,750 from Myers and his relations.

Meanwhile work had been progressing at Mill Lane. In June 1909 the laboratory was presented with a "recording dynamometer" and an "automatograph", as well as with "apparatus for the study of visual after-images and simultaneous contrast and apparatus for presenting audition and visual stimuli for use in reaction times". In the same report, written by Myers, for the Museums and Library Syndicate (see Reporter June 1909), he notes that his old tutor and friend, Sheridan Lea, had devised and constructed this equipment himself. Myers' research report for 1910 records that Mr. Bullough had been working on the aesthetics of colour perception, Mr. Rusk on association tasks, E. M. Smith on the colour sense of dogs and Miss Tucker on the colour sense of children. Myers' last report for Mill Lane appeared in 1912. He noted that over 40 people had been using the facilities, eight of them being actively engaged in research. He also notes that the new laboratory will help bridge the gap, "which exists only in this country" between experimental and general psychology. To a description of the new laboratory and the early work carried out there we now turn.

D. The New Laboratory

The "New Laboratory" still houses Cambridge psychology. This and the considerable advance the building constituted over Mill Lane indicate that it was a

considerable achievement - indeed it was probably as good as anywhere in the world.

Myers gave a speech at the laboratory's official opening in which he expressed his hopes for the future of Cambridge psychology. (The speech still exists, in typescript form, in the laboratory's archives). He stressed the strong connections of physiology to psychology. But he put far more emphasis on applying psychology. He said,

> "In its applications Psychology enters into relation with Biology, in the study of animal behaviour; with Education, in the study of the individual and general characteristics of the developing human mind; with Economics, in the study of the best methods of securing mental and muscular efficiency within the community, and of the relation between mental endowment and fitting occupation for the individual; with Anthropology, in the study of racial and mental differences; with Medicine, in the study of the disturbances of the nervous system and sense organs, and in the use of suggestion, hypnotism and psychoanalysis as therapeutic measures; with Theology, in the study of the intellectual and emotional factors in religion; and with Art as a foundation for experimental Aesthetics".

Having got his laboratory built it is crystal clear from Myers' words in what direction <u>he</u> would have liked psychology to progress.

The actual building more or less conformed to the original plan drawn up by Ward, Rivers and Myers in 1910 (reproduced earlier). Bartlett in the Cambridge Review November 7th 1912 wrote,

> "The new building consists of three floors. The ground floor contains a lecture room, office, assistant's room, mechanic's workshop, and a room for Animal Psychology, leading out into what will be a grass court. On the first floor are a library, well-lighted

practical class rooms, and a dark room. The six rooms on the second floor are solely for research. Here a sound-proof room is in course of construction, similar to those existing (or being erected) in the Laboratories of Utrecht, Groningen, Frankfort and St. Petersburg.

With the consent of Professor Langley and the Drapers' Company, the heating, lighting and electric power are to be drawn from the installations provided in the Physiological Laboratory. Two 4-volt and one 110-volt circuits are distributed throughout the building, and a separate high-pressure circuit has been installed in several rooms for the use of arc-lights, for driving powerful motors, etc.

A small engine and organ bellows, driven hydraulically, have been installed in the workshop. The engine can be started from various rooms in the Laboratory. By its means air can be supplied at a constant pressure for the blowing of instruments for acoustic purposes.

The cost of the building and its equipment has been met by sums collected by the Director of the Laboratory and by the University Association. More recently a grant for apparatus has been made by the Museums and Lecture Room Syndicate.

The new Laboratory will certainly rank as one of the best of the many buildings in Europe and the United States devoted to Experimental Psychology."

The most interesting feature of the building was probably the soundproof room. This was built especially suspended on two girders let into the walls to allow for all round insulation. Based on York Stone the walls also contained concrete, cork and chalk with the final insulation being provided with a layer of compressed peat and horsehair.

A good idea of the capacity of the new laboratory is given in the programme of events for an open day held on

June 9th 1914. Seven demonstrations were staged namely:-

1. Discrimination of Light and Sound, "as employed in investigations into the individual mental differences of guinea pigs and the inheritance of those differences".
2. "The Maze" - to demonstrate animal learning, the extent of persistence after rest, reversal training spontaneous learning of short cuts etc.
3. "Association Times" - to show the different reaction times for different sorts of association, using a Hipp Chronoscope and "lip-key" capable of measuring thousandths of seconds.
4. "The Memory Drum".
5. "Line Drawing" - to show motor learning under conditions of visual adaptation.
6. "The Sound Proof Room" - to show the effect on localisation of harmonic changes.
7. "The Tachistoscope" - to demonstrate the nature of visual illusions with no eye movement, the gradual perception of a picture and the span of attention.

It can be readily seen that the new laboratory offered considerably increased scope for both teaching and research in psychology. Soon after its opening Smith continued her work with animals and Myers his with synaesthesia, tone and localisation. Bartlett began his publishing career with "An Experimental Study of Some Problems of

Perceiving and Imaging". (Bartlett 1916(1)).

In July 1913 in his annual report to the General Board of Studies Myers declared that the lack of any assistant to himself had caused the curtailment of teaching, the cancellation of courses and the transfer of pupils. On the 30th of September Cyril Burt was appointed as the new "Assistant in Experimental Psychology" and was paid £175 per annum from University funds.

In fact 1913 saw a considerable increase in the scope of the psychology taught at Cambridge. Myers, with Dawes Hicks and Burt, bore the brunt of this increased work load which involved far more practical work than before. However, Adrian (later the renowned physiologist Lord Adrian), was called in to lecture on the "Structure and function of the nervous system and sense organs". Meanwhile Moore and Ward were still called upon to teach their more philosophical approach to psychology.

However all this activity was brought rudely to a halt in 1914 by the outbreak of war.

E. The Laboratory and World War I

As we have seen Rivers and Myers became involved with clinical psychology at Maghull Hospital, and elsewhere, very soon after the outbreak of war. Bartlett, who had gained his first in the Moral Science Tripos was appointed as Myers' assistant in June 1914. This post was left vacant by Cyril Burt who was soon to join the London County Council.[1] By November 1915 Bartlett was left without a senior in Cambridge. Even Bernard Muscio, who had taken

(1) Burt's L.C.C. post was in fact only a "half-time" appointment. See also page 224.

Lewis' post as Demonstrator in 1914, left during 1916 to return to his native Australia. Thus on the 20th of November Bartlett was appointed as "Interim Director" of the laboratory until Myers returned.

Teaching and research muddled on as best it could during the War. But nothing of real academic importance emerged. In the first year of War 26, 34 and 24 pupils were instructed in each of the three terms. By 1916 these figures dropped to 13, 12 and 9. In his annual reports from the laboratory Bartlett had very little to comment on other than on E. M. Smith's continuing research on animals and the beginning of his own studies on perception and imagery. In 1917 Bartlett was elected a Fellow of St. John's on the strength of his dissertation which was entitled "Transformations Arising From Repeated Representation. A Contribution Towards an Experimental Study of the Process of Conventionalisation". This paper was the direct forerunner of Remembering and contained discussion of many of the experiments which later appeared in that book. This work must have occupied most of Bartlett's time during the War.

However at the same time the laboratory staff were invited to give what help they could at the Eastern General Hospital, Cambridge. Bartlett later wrote (1969).

> "to this there came a number of cases of what was then known as 'shell-shock'. There was no properly trained medical personnel available for dealing with these cases, and as a psychologist with practical interests I was invited to see what I could do to help them. I had discussed current treatments of 'shell-shock' with Myers and Rivers and, like all psychologists of the period I had read

> everything I could get of the work of Sigmund Freud..... I cannot say whether what I was able to do was of much, or indeed of any, use to these patients: none of them was allowed to stay long in Cambridge. But I myself learned two lessons..... The first concerned the value to the psychologist of collaboration with medically trained experts, and the second that psychological insight and understanding always demand a consideration of the internal, and personal, conditions of behaviour as well as a study, and if possible, a control of external circumstance".

This episode then clearly had a considerable impact on Bartlett himself. It also served to consolidate the link between Cambridge psychology and medicine - a link which, after the War, led to much clinical research in local hospitals by L. G. Fildes.

But the laboratory's main contribution to the War came at the very end. It is described adequately by Bartlett himself, (op. cit.).

> "A second, and extremely different set of problems soon presented themselves. The adoption by Germany of a policy of unrestricted submarine warfare became an increasing and dangerous threat to British and Allied shipping. Early in 1917, I believe it was, a number of businessmen formed the Lancashire Anti-Submarine Committee with Professor (later Lord) Rutherford as its Chairman. Enemy submarines then had to be detected by listening over hydrophone circuits, and their direction of travel identified. The essential qualifications required by anti-submarine personnel were acute hearing, a capacity to identify the pitch, quality and rhythm of the sounds emitted by enemy submarines, ability to localise these sounds accurately and without delay, and to detect any increase or decrease in their intensity. Decisive and rapid reaction were also demanded. These qualities are unevenly distributed in any unselected human population, but their presence and potential effectiveness in any individual can be determined by experiment. Of the capacities needed, accurate sound localisation was of leading importance. Charles Myers and H. A. Wilson had published an important

paper on auditory localisation, and T. H. Pear (himself fully occupied in other forms of war work), hearing of the requirements of the L.A.S.C., pointed out this paper to its Secretary who at once made contact with Myers. Myers, in his turn, suggested that I might take charge of a Unit to develop and apply special selection test volunteers for British anti-submarine operations. This I agreed to do and, strongly backed by the L.A.S.C. and also by the Admiralty, the Unit was speedily formed and got to work in a new anti-submarine training school at H.M.S. Crystal Palace. The Selection Group consisted of two experimental psychologists, two musicians of eminence and a physicist. The Service officers belonging to the training school were very friendly and helpful. So far as could be judged by recorded results in terms of the sinking of enemy submarines our early selections were significantly successful, and before long it was officially laid down that all Officers and Ratings who volunteered for this Service should present themselves for our tests.

Already, as we knew, America had developed special tests, mostly of intelligence, for general army purposes, and these were in large-scale operation, but the Crystal Palace selection group was, I believe, the first organised attempt in this country to employ psychological selection methods. It was pretty well bound to be directed from Cambridge because of Myers' earlier experimental work, and because the laboratory he had designed and built was the only one then available properly equipped for the kind of experimental trials that were necessary. The enterprise had two vastly important implications. The first was that it strongly re-inforced the view that experimental psychologists must be prepared to work together with experts in other, perhaps many other, fields. The second was that experimental psychology might well itself contribute fundamentally to developing technological knowledge and technical practice".

Bartlett with E. M. Smith wrote up some of this research under the title of "On Listening to Sounds of Weak

Intensity" (Bartlett 1919). These two papers describe their attempts to refine the standard apparatus of the time, the Politzer acoumeter. They appear to have had some success in achieving in their dual aims;

1. to devise apparatus and methods for a satisfactory auditory acuity test.
2. to observe the objective and subjective factors involved in detecting sounds of weak intensity.

All this work was of course performed in the new sound proof room. After the War had ended in 1918 normal progress was gradually resumed at Cambridge and psychology began to pick up from where it had been so rudely interrupted.

F. Post War Developments 1918 - 1922

As we have seen Rivers and Myers both returned from the War as changed men. In 1919 Rivers formally resigned his lectureship. Hamilton Hartridge took over the post. J. P. Lowson took over the Demonstratorship which had lain vacant since Muscio's departure in 1916.

Myers, in this period, spent much of his time organising the N.I.I.P. in London. Bartlett was left to shoulder the burden, taking over many of his courses. However courses in psychology resumed in 1919 more or less where they left off in 1914. There were two interesting additions. Bartlett himself began to lecture on "Psychology in relation to Industry" and a Mr. Hope began the first

Cambridge course on "Personality". Research however had suffered during the War and little of importance was done for some years afterwards. This was in no small part due to the various diversions which had attracted Rivers and Myers, namely anthropology and industrial psychology respectively. Bartlett too, at this time at least, seemed to be far more interested in the psychology of anthropology than in anything else. In fact after his "Sounds of Weak Intensity" paper he only published two more experimental studies before 1925 when the material for Remembering began to appear.

The next step of interest occurred in November 1919. Then the Board for Moral Sciences recommended that a Board of Psychological Studies be set up. In a report signed by Ward, Sorley, Moore, Johnson, Dawes Hicks, Bartlett and C. D. Broad they stated, "during the last few years, partly owing to the War and after-war conditions, a great impetus has been given to the study of psychology". It had been shown, the report goes on, that psychology could be of the utmost importance particularly for "mental disorders and problems of education and industry". The report also pointed out that psychology had "greatly advanced" developing branches involved with physiology, sociology, religion, aesthetics and animal behaviour. The discipline had become too specialised and technical for adequate supervision by the Moral Science Board which, in addition, now considered itself inadequate to examine candidates in psychology.

Bartlett spoke in favour of the proposal when it came to the Senate. He emphasised the need for an independent board to maintain the current growth of psychology - especially in its practical applications. Students were being attracted elsewhere he warned - especially to Manchester where T. H. Pear held the chair and psychology was taught as a science subject. The proposal was duly carried on 12th March 1920.

Myers was the Board's first chairman and Bartlett its secretary. Hartridge, Rivers, Sorley, Adrian, Lowson and Dawes Hicks were also at the first meeting on July 17th. Very soon the Board was firmly pressing two points. Firstly it "reminded" the boards for medicine, teaching, economics and politics that psychology should form an indispensable part of their courses. Secondly the Board decided to campaign for a "Readership in Psychology" and that Myers should be appointed to the post.

The General Board endorsed the proposed Readership, pointing out (Reporter 1921 p. 324) that under Myers psychology had flourished - especially in its wartime, medical and industrial applications. They proposed that £650 per annum should be alotted to the post from the Medical Grant Fund. The proposal, thus endorsed, went to the Senate on January 28th 1921. There the proceedings were spoilt a little by the demand of the Moral Science Board that the post be called a Readership in <u>Experimental</u> or <u>Applied</u> Psychology. This demand was led by James Ward who argued that Cambridge psychology had become almost entirely experimental and applied - there being very little of what Ward saw as <u>pure</u> psychology remaining. As Bartlett

puts it (1969), "He regarded all the experiments we did as psychophysics rather than as proper psychology". This was Ward's last significant appearance in psychology. He gave no more lectures after 1921. Thus Myers became "Reader in Experimental Psychology". Myers did not really mind the change of title - he referred to these proceedings later as "a rather silly little dispute". However the incident does illustrate Ward's positive disenchantment with the way psychology developed towards the end of his life. It also illustrates the fact that, perhaps because of this, Myers and Ward never really got on all that well with each other.

Meanwhile psychology continued its post war expansion on other fronts. Over 30 courses were run each year. Over 80 students used the laboratory each term. Myers reported to the Board of Psychological Studies (whose Minutes are still available in the laboratory archives) that as early as 1920 "the laboratory is being taxed to its utmost resources to find sufficient room for research students." Courses were begun in abnormal psychology, mental testing and in comparative and social psychology. In 1920-1921 Cambridge also began, in earnest, its later prolific export trade in psychologists. Thouless, who had recently received his Ph.D. and given lectures on the "Psychology of Religion," went to a lectureship at Manchester. Muscio, who had been working at Cambridge for the I.F.R.B., as well as giving lectures on "Industrial Psychology," left for a Professorship in Sydney. Mr. Prideaux and Miss L. E. Fildes at this time had begun the laboratory's long association with the Addenbrooke Hospital and

had begun also to liaise with the judiciary on such issues as criminal responsibility.

The minutes of the Psychological Board for the years 1920-1922 are very revealing of the way its members wanted psychology to develop. In a report to the Royal Commission investigating the University they recommended "very strongly" an expansion of the staff. They foresaw the need for lectureships in Industrial, Abnormal and Educational Psychology. These posts, together with a Chair in Psychology, were priorities, they claimed. In November 1921 they started their fight to establish psychology as a science subject and thus as part of the Natural Science Tripos. This fight, as we shall see later was to take many years and was not in fact won until 1934.

Thus everything looked very rosy at this time. Then early in 1922 Myers announced his impending resignation. His post had been created ad hominem - there was a real danger of the Readership departing in Myers' wake. However, Myers characteristically engineered the situation to his own satisfaction. A review of the psychology staff was undertaken by the General Board. They accepted the Psychological Board's twin recommendations that:

1. Bartlett's post of "Assistant" should be abolished and Bartlett should replace Myers as Reader and Director of the laboratory at the same salary of £650 per annum.
2. That the £175 per annum thus freed should be used to provide a new "Lectureship in Psychopathology" to ensure the future of the Diploma in Psychological Medicine.

Bartlett (1969 and 1965) has expressed his gratitude to Myers for securing him the post. But it suited Myers very well to have his protege succeed him and to secure the future of his Diploma in one swoop. These proposals, not surprisingly, passed through the Senate and its Financial Board without dispute and Myers, a no doubt satisfied man, left Cambridge for the N.I.I.P.

Almost immediately, on the 4th June 1922, Rivers died. Eight days later Bartlett was officially appointed the Readership. Thus at the age of 36 Bartlett found himself without a senior in psychology at Cambridge. He inherited, though, as we have seen a flourishing, active and expanding entourage - largely thanks to the sterling efforts of Charles Samuel Myers.

CHAPTER 10

THE CONTRIBUTION TO PSYCHOLOGY OF RIVERS AND MYERS

A. Rivers' Psychology

Rivers was arguably Britain's first experimental psychologist. His first experiments were on vision and were carried out in the one room allotted to him in the Physiology Department in the years 1893-1897. The other areas in which Rivers contributed significantly to experimental psychology were to be fatigue (especially in relation to drugs) and his work with Head on the nervous system. But to Rivers' first interest, vision, we now turn.

Rivers' contribution to vision is perhaps four-fold.

1. His encylopaedic article in Schafer's Textbook of Physiology was described by Myers (1922(2)) as "the most accurate and careful account of the whole subject in the English language". In it he placed especial emphasis on a discussion of the competing theories of Hering and Helmholtz. He pointed out the difficulties involved with the latter's account of the phenomena of successive and simultaneous contrast. Rivers claimed that both were due to physiological factors namely the interactions between adjoining retinal areas. However his main achievement here was to present in English an account of previous perceptual theories - it was an account on which British researchers had to rely for at least a quarter of a century.

2. Following on from his Torres Straits investigations Rivers, with Dawes Hicks, initiated the study of visual illusions in Britain (for details see next section "The Torres Straits Expedition").
3. He began the British studies of apparent size or "perceptual constancy". As early as 1896 he published an article describing the effects of the drugs atropin and eserin on this phenomenon. His results very strongly suggested through the similarity of effects of drugs, eye focussing and eye movement that a central mechanism was responsible.
4. He also began cross cultural studies of sensation and perception - but again see next section on the Torres Straits.

In essence then Rivers formally started perceptual research in the U.K. He started it off in the three experimental areas outlined above and provided British psychology with the necessary theoretical background for the work of others to progress.

Rivers was also a pioneer of the experimental study of muscular and mental fatigue especially in relation to caffeine and alcohol. His interest in this area began with his work with Kraepelin in 1893, which was published as "Observations on Mental Fatigue and Recovery" in 1896. His interest concluded when he finished fifteen years of

work in this area with his 1908 Croonian Lectures on "The Influence of Alcohol and Other Drugs on Fatigue". These lectures were mainly based on a remarkable piece of research which Rivers conducted in the two previous years. Firstly he found himself a suitable subject - a Mr. H. N. Webber. This must have been fairly difficult, for Rivers required his subject to abstain for a year from the drugs under study - alcohol and caffeine. Rivers himself also abstained from these drugs for an unspecified time. A second control innovated by Rivers was the disguise of the drug to make it indistinguishable from a control solution. Thus Rivers hoped to avoid the contaminating factors of habituation and the stimulation, excitement and/or interest evoked by even the idea of alcohol and caffeine. Surprisingly enough such elementary controls had not been employed by previous researchers. Probably using the ergograph at Mill Lane (photograph in Appendix) he took several sets of readings of muscular efficiency every day from Webber usually administering the drug (or control) after the first set.

Rivers' results were entirely contrary to those previously noted. Alcohol tended to diminish muscular and mental efficiency he concluded by suppressing central control. The effect, however, of similar doses of whisky was often to increase muscular work immediately. This strongly suggested the importance of sensory and cognitive factors. He also found much less pronounced results, than had other workers, with caffeine. Thus Rivers had introduced both a new area of study and had emphasised

the crucial importance of careful, controlled experimentation. He concluded his Croonian Lectures by pointing out the possibility of applying drug techniques to the study of individual differences - especially to investigate physiological variations.

The third area of important psychological work Rivers became involved in was his study of the nervous system with Henry Head. It is rather hard to disentangle the twin contributions of Rivers and Head towards their famous 1908 paper "A Human Experiment in Nerve Division". On the other hand it is relatively easy to assess the importance of the work and to admire their dedication. The experiment began in 1903 when Rivers divided "the radial and external cutaneous nerves" (Head 1923) in Head's left arm. For five years, in their spare time, Head underwent this painful procedure in order to observe the loss and return of sensibility thus incurred. Both men, influenced by Hughlings Jackson, postulated three differentialy developed levels of the nervous system on the basis of their results. These were a deep crude system responsive only to pressure and movement but more importantly a protopathic and an epicritic system above this. The protopathic was responsible for the detection of the presence or absence of a particular sensation, such as temperature or pain and was only crudely localised. The epicritic was the most highly evolved and was responsible for fine discrimination of intensity and localisation.

As Thompson (1967) puts it the current consensus of opinion regarding Rivers and Head's formulation is that the concept of epicritic and protopathic systems is a

useful one. Thompson adds (p. 233) that of course current knowledge is far more refined. Rivers and Head also went way beyond their evidence which only related to cutaneous nerves. However they were by and large correct in their treatment of cutaneous sensation. As Thompson puts it there are now known to be two specific or "epicritic," receptor types for fine touch while "all other skin afferents come from nonspecific fine nerve plexes in the skin". Thus the original distinction still holds for cutaneous nerves a finding supported by Deutsch and Deutsch (1966) on the basis of their own cutaneous nerve lesion experiments.

The findings also proved to be a most stimulating source of research for years to come. Especially so when Head went on to apply his "levels" theory to neurological subject matter. Head, for all his errors, was a major figure in British neurology. Rivers was involved in the very origins of his ideas.

This idea of different levels of nervous development reoccurred in Rivers' thought when he came to interpret Freud. He saw a parallel between Freud's view of repressed instincts (held in check by a sophisicated system of censorship and super ego) and the functioning of the protopathic nervous system (the gross impulses of which were held in check by the epicritic system). But we return to this later.

B. The Torres Straits Expedition

As we have seen Haddon's Torres Straits expedition marked something of a turning point in the lives of both

Rivers and Myers. Their work convinced Myers of the fruitfulness of psychophysical investigation of the senses and instilled in Rivers a newly found enthusiasm for anthropology. But the expedition also marks an important event in British psychology - quite apart from these personal effects on two of its founders.

Firstly the expedition founded a new branch of psychology which has been with us ever since - namely cross cultural psychology. Under Rivers'direction the Torres Straits psychologists brought back a wealth of data on the sensation and perception of the native peoples they examined. This data was of intrinsic interest to psychologists for the new light it shed on perceptual processes. The data was also of immense value to ethnologists for, as we shall see, Rivers' data shed light on the anthropological theories of the day. Thus the expedition's legacy was to firmly establish cross cultural or comparative psychology as an interesting field in itself and also to demonstrate psychology's usefulness and relevance to anthropology.

More particularly just what did the expedition do in the psychological field? In general terms it demolished the then popular myth of the noble savage. Psychophysical differences when they <u>were</u> found were usually very small. Natives did not have superhuman visual or auditory acuity. They were also quite capable, again contrary to popular expectation, of concentrating sufficiently on laboratory tests. Thus the differences noticed in

memory tests, reaction times, mental fatigue and muscular efficiency were noticeable by their almost total absence. Similar results were found in tactile and olfactory investigations. The only real differences were found in the visual field. The fact that they were found probably was not a coincidence. Rivers' own interests were, at this time, firmly with the study of vision and especially colour vision. His knowledge of experimental technique was also best in this area. Thus the procedures adopted for visual experiments were probably rather more sensitive than those employed for the other senses. The main differences observed in the Torres Straits were:-

1. Colour blindness, in general was far less frequent among natives than for European peoples. There were also marked differences between different native tribes. Myopia and other visual difficiencies were also found to be less common.

2. Rivers also claimed to have discovered a generally lower level among natives of sensitivity to blue. However he also reported a "generally defective nomenclature for blue, green and brown". In fact Rivers discovered that many Papuans possessed no word for blue and were quite happy to apply the same word to a blue sea or a black sky.

   Rivers findings thus raised the problems of what came, latterly, to be called the Whorf hypothesis. The problems were of course

whether the observed diminution in sensitivity to blue was a true physiological or even phenomenological effect or whether it was merely an artefact of labelling. Myers (1911(2)) firmly supported the labelling theory. Rivers was no way towards solving these problems but he was one of the first to raise them and they have been important ever since in cross cultural studies of vision.

3. Rivers discovered that among natives the vertical - horizontal line illusion was more pronounced while the Müller-Lyer illusion was less marked. Rivers explained this in a way similar to that later favoured by the Gestaltists. Natives, he claimed were less used to perceiving complex and often angular figures as whole units. Thus they were far less likely to perceive the line with outward pointing arrows as a discrete whole and thus less likely to perceive it as contrasted. It is probably fair to say that there is still no one satisfactory theory of visual illusions. A good discussion of competing theories is however provided by Gregory (1966). Rivers' theory is still with us today.

The vertical - horizontal line illusion, Rivers held, was due to the greater difficulty of up-and-down over side-to-side eye movements. This led, he argued, to an overestimation of the length of the vertical line. Thus the differences observed in the Torres Straits, he claimed, were due to physiological factors. In 1908 Rivers followed this work up with an experiment with Dawes Hicks. Using tachistoscopic presentation of the illusion they hoped to eliminate eye movements. But the illusion persisted - in fact producing surer and quicker answers. This, in turn, led Rivers to the view that although eye movements were necessary for the generation of visual space perception once this was learnt eye movements were not necessary for its continuance. Thus Rivers' Torres Straits data led to this not insubstantial contribution to perceptual theory - as indeed cross cultural data has done ever since.

That in essence was the specific contribution of the Torres Straits expedition. But as Hearnshaw (1964 p. 173) concurs its more general effects on the outlooks of Rivers, Myers and thence Bartlett was of inestimably greater importance as indeed was its role in establishing British cross cultural psychology.

C. Rivers' Anthropology

Rivers' contribution to anthropology was of considerable significance - although it is only of passing interest here. His anthropological work falls into three, chronological, categories. He worked firstly with the Todas, then in Melanesia and lastly concerned himself with theoretical problems, in which the assimilation of Freudian theory figured prominently. A fairly representative selection of Rivers' anthropological writings was published posthumously in 1926 under the title of Psychology and Ethnology.

As we have seen, on the Torres Straits expedition, Rivers conducted, quite separately, cross cultural studies of sensation and perception and studies of the natives' culture. He repeated this pattern in his 1901-1902 investigation of the Todas. Thus in 1905 he published his "Observations on the Senses of the Todas" which was a piece of straightforward physiological psychology. In 1906, in his book The Todas he described the customs, beliefs and general culture of these people. The book was a valuable contribution recording as it did the behaviours and social structures of tribes fast being influenced by European culture. However the work was based on the then predominant dogma of psychic unity - a theory of which Rivers was later to be the most eloquent critic. Hence, as he was the first to admit, the work had its limitations.

These limitations are best illustrated by looking at the next stage in Rivers' work - his studies of Melanesian culture. For it was these that led him to his influential repudiation of psychic unity.

This repudiation came in 1911, after Rivers' first visit to Melanesia. It formed his Presidential Address to the Anthropological Section of the British Association and is reprinted as "The Ethnological Analysis of Culture" (in Rivers 1926). The theoretical background implicitly assumed by British anthropologists, he pointed out, was that propounded, in Germany, by Adolf Bastian. His evolutionary, independent origin or psychic unity theory held that human beings were all essentially psychologically similar. Hence similar social customs, magical rites, child rearing practices and indeed the whole social structure would develop totally independently given the same or similar physical conditions. This so called evolutionary theory was barely questioned, in Britain, before Rivers - as Haddon (1922) points out. Yet it was an extremely dangerous theory. For firstly as Rivers pointed out (1926) it had blinded him to certain elements of culture which he had been led to regard as unimportant. Secondly the theory was so vague as to be irrefutable and yet, as had happened to him, confirming evidence veritably leapt to the eyes wherever one looked.

Rivers himself (1906(1)) wrote,

> "Perhaps the most definite result which modern research in Anthropology has brought out is the extraordinary similarity of custom throughout the world ..... Customs apparently identical are found in races so widely separated geographically and so diverse ethnologically that it seems certain the customs must have developed in total independence of one another".

Yet after his trip to Melanesia, he became convinced, through his careful ethnological observation and analysis

of these various cultures, of the existence of waves of immigration in the past. He found clear evidence, he claimed, for the effects of culture contact, diffusion and transmission. The lessons for anthropological method which he drew from this were:-

1. That careful and systematic ethnological observation - with a due respect for language and labelling problems should come first - and analysis and theorising afterwards.
2. That ethnological studies should be regarded as more important than the previously popular studies of material culture. For Rivers himself had shown that swapping and mixing of material culture occurred under far more superficial conditions of contact than those required for any significant changes in custom, belief or social structure. Thus as he put it (1926 p. 133), "material objects are the least trustworthy of all the constituents of culture".
3. That therefore social psychology had an important role in anthropology and the close links between the subjects should be recognised.

Thus Rivers was an important factor in de-mystifying anthropology. He dragged it away from its previous shaky

philosophical foundations and placed it firmly among the observational sciences. His approach, in fact, had much in common, with that of Bastian's countryman Ratzel who was also propounding the ethnological approach as it came to be called.

So, before the first World War Rivers had made two important contributions to anthropology. Firstly he collected masses of data on many aspects of fast disappearing cultures. Secondly, and probably of more importance, he spearheaded a considerable advance in anthropological method and theory.

The third stage of Rivers' anthropological career began during the War and then monopolised his interests until his death. This stage of his work was inspired by the advent of Freud. Rivers characteristically was stimulated by Freud's ideas - while being apalled at the rather sweeping and mystical nature of the theory. He devoted much of his effort, in the last period of his life, to thinking out the implications of Freudian theory for psychology and ethnology. This effort bore fruit in his works Instinct and the Unconscious (1920), The Symbolism of Rebirth (1922), Freud's Conception of the Censorship (1920) and The Repression of War Experience (1918). The basic point is that Rivers noted the opportunity afforded by Freud to engineer a fusion of ethnology and psychology. His argument was best presented in his two Presidential Addresses to the Folk-Lore Society of 1921 and 1922. Ethnology was far more he claimed than a "collection of curiosities". Comparative study of

human belief and custom provides material for a history of human progress not merely in material terms but rather

> "a history of the movement of thought; of the long struggle of Man with his environment; and of the countless institutions, beliefs and customs which have been the outcome of this struggle".

Thus ethnology provided material for the psychologist - who after all is interested both in individual and social behaviour. The connection is strengthened, claims Rivers, when one recalls that all social facts are, in the end, the result of mental activity.

Most importantly ethnology can provide data on the usage of symbolism - an issue of course then popularised by Freud and Jung. Symbolism had been shown to be of importance in dreams and mental disease but the main issue was, surely, that of the universality of symbolism. Freud and Jung seemed, to Rivers, to be claiming a sort of psychical unity for all mankind. This meant that all humans had a propensity, probably genetic, to use symbols and moreover to use them in similar ways. But, as Rivers points out, even if a symbol was found to be universal, it would not solve the problem of how it came to be so. However it would show that at the very least human beings found such a symbol easy and natural to use.

As ever the way out of the dilemma was through observation. He took just one instance, the symbolism of rebirth often used in religious ritual. He pointed out that such symbolism had been shown by ethnological study

to be by no means universal and furthermore that its occurrence could be explained by the usual studies of population movement and transmission of culture.

Essentially then Rivers marshalled similar arguments against Freud as he had earlier against Bastian - indeed the two theories are very similar in some respects. A further consideration of Rivers' treatment of Freud, in its more psychological context, is given below. Meanwhile his contribution to anthropology is neatly summarised by G. E. Smith in Rivers (1926). Of this demolition of Bastian and Freud's wilder claims he wrote,

> "But if this is the most signal service rendered by Dr. Rivers it by no means represents the whole debt Ethnology owes him. He gave the subject scientific discipline, breadth of outlook and coherence with humanitarian studies. He established the intimate co-operation of Ethnology with Psychology - not the spurious psychology of Bastian's ("psychic unity") or Freud's typical symbols but the serious study of mental phenomena by methods conforming to scientific principles".

D. Myers' Experimental Psychology

Myers did not wish to be judged solely on the basis of his experimental work. "I certainly got more interest and pleasure," he reflected in 1942, "in planning the researches of others and in interpreting and co-ordinating the results of new research than in prosecuting research myself. Organising and administration always occupied and interested me - all on behalf of psychology". As we have seen in the previous chapters he was remarkably successful in doing this. Myers is that unusual case of a

man who never made any earth shattering experimental advance or even put his name to any revolutionary theory and who, nevertheless, occupies a massive place in the history of British psychology.

Bartlett (1965) usefully divides Myers' work into "expos itory," "experimental" and "ethnological". The last category concerns his cross cultural psychophysical work on the Torres Straits and has been described above.

Myers'expository work was of first rate importance. In 1909 he published his Textbook of Experimental Psychology and followed it two years later with the briefer and livelier Introduction to Experimental Psychology. The former book ran to three editions finally appearing in 1931 while the latter ran to four, the last being published in 1919. They remained standard, indeed almost compulsory, texts for any new student of psychology for over twenty years. Bartlett remembered being introduced to these books, he wrote (1965),

> "This was pioneering and genuinely original work to an extent which it is now difficult to realise. The great bulk of what was then regarded as of any importance in experimental psychology was written in German, usually at immense length and in a very difficult way, and was untranslated. There was no single book in English with which the ordinary student had much chance of making headway".

Myers, Bartlett goes on, had "read everything" - which his fluent German helped him to do - and "exercised a most extraordinarily correct power of selection".

A glance through Myers' Text Book only serves to confirm Bartlett's opinion. In the third edition, Part I

presents the experimental background to; auditory, cutaneous, visceral motor, visual, gustatory and olfactory sensations; reaction times; memory; psychophysics; binaural and binocular experience; emotion; attention; visual perception of size and direction; and lastly time and rhythm, as well as dealing with statistical methods. Part II, refined for the third edition by Bartlett, is an exhaustive account of the various methods and demonstrations which can be applied in all these areas. It was an extraordinarily thorough book and it is probably true to say that it only ceased to be printed because it rapidly became increasingly difficult for any one book to cope with the whole of the growing field of experimental psychology.

Turning to his experimental work we note that this falls into two areas, work on auditory localisation and on various aspects of music.

His work on auditory localisation was important for two reasons. Firstly it provided the basis for the military hydro-phone work described in Chapter 9 . Secondly, it began a fertile area of research within British psychology. Myers in 1908(2)) claimed that auditory localisation occurred through the detection of minute phase differences (which occurred through the slightly different positions of the ears) and through the detection of intensity differences (through bone conduction).

The work was of importance to the war effort because Myers had shown there were large individual differences in localisation ability and later, in 1914(1)) he had shown

the effect of pitch and loudness on this ability. As we have seen earlier, on this basis, Bartlett was able to perform useful selection work for H.M.S. Crystal Palace for operators of hydro-phone equipment.

On the more academic side in the 1920's Harry Banister followed up Myers work with the benefit of improved equipment and the use of better controls. He found that localisation even of pure tones could occur by phase differences alone. Intensity differences played a smaller role, he claimed and time of arrival differences a larger role than Myers had supposed. Thus Myers probably did get it wrong. But at the same time he had invented a methodology for this problem which was adopted and bore fruit in later years. Indeed as Hearnshaw (1964) has pointed out it was largely through the efforts of Myers and Banister that the British auditory work of the 1920's was recognised by Murchison in 1934. Murchison in his Handbook of General Experimental Psychology invited Banister and Hartridge to be the principal contributors on audition - they were the only British workers to be so asked.

Myers' work on music was diverse in scope but never really came together to form a coherent scheme. His early work was ethnological - comparing natives and native cultures with respect to; different types of music; instruments; appreciation of tone and rhythmn; and the role played in the society by music. Later he postulated various types of musical appreciation - work which stemmed from the work of Bullough (which Myers had instigated) on aesthetic appreciation.

In this latter area perhaps the most representative paper by Myers is that of 1921 entitled "Individual Differences in Listening to Music". But Bullough's papers[1] of 1920 and 1921 and Myers' of 1914 are also good examples of this line of work, most of which appeared in the British Journal of Psychology.

Overall Myers went along with Bullough's concept of "psychical distance". Myers writes (1921(1)p 20)

> "The one common and essential attitude required for aesthetic enjoyment is one of detachment. The listener must view the music, as Bullough rightly insists, from a certain psychical 'distance'. If that distance be excessive, as occurs in listening for the first time to exotic music or to other unfamiliar styles of music, the subject feels too remote to get, as it were to grips with the art material. It is over distanced. On the other hand, it is under-distanced, when he surrenders himself wholly to its influence in such a way that he is a more or less passive instrument played upon by the music".

Also following Bullough, Myers concluded from his studies that aesthetic appreciation could be categorised as;

1. Sensory, emotional or conative experience.
2. The association which the music aroused.
3. Its value as an object - for music its technical excellence.
4. The nature of the characterisations and anthropormorphisms which the piece evoked.

Myers' method was to question subjects regarding their reactions to selected pieces of music. He asked about th eir analysis of it and the introspections which it provoked.

(1) Bullough also published on this theme in British Journal of Psychology. Vols. 2,3 and 5.

His work was thorough and deep. It is obviously open however to criticisms of interviewer bias non standardised questioning and so on. However Myers seemed to see his work as being more exploratory than definitive and to some degree he did add to this very embryonic - and difficult field of study.

E. The "Shell Shock" Controversy and Freudian Theory

Background

Rivers and Myers became involved in the interconnected issues of shell shock and Freudian theory during the War. Myers was the more prominent in the former and Rivers in the latter. The shell shock controversy was touched upon in Chapter 8. It concerned the growing campaign by psychologists and indeed some medical doctors for the recognition of the psychological nature of some categories of insanity in general and shell shock in particular. The advent of Freudian theory began to attract psychologists' attention in 1913 when T. H. Pear and Ernest Jones published articles in the British Journal of Psychology. In the years that followed psycho-analytic theory was a constant source of controversy and dispute, as the many articles in the British Journal show. Freud of course provided a considerable shot in the arm for those campaigning for the recognition of psychological factors in mental illness. The Medico-Psychological Association had, in fact, before the War, helped to institute a Diploma in Psychological Medicine at Edinburgh, Durham, London and,

as we have seen, at Cambridge. But the prevailing attitude within British psychiatry was far from helpful.

As Hearnshaw has put it: (1964 p. 282)

> "Today there is a recognised role for the application of psychology and for the clinical psychologist in the field of mental health. Though the possibilities of such a role had been envisaged,..... even in the nineteenth century, it was for practical purposes not much more than a dream until after the Second World War".

He continues of this time that, (p. 287),

> "There was among British psychiatrists, if not anything as definite as a school of thought, a proclivity to prefer the objective approach to the study of mental disorder; to agree with Mott as to the importance of genetic influences; to stress the role played by toxic and biochemical factors in the aetiology of the psychoses; and, when in the 1930's physical methods of treatment (convulsion therapy, insulin therapy, leucotomy, etc) were introduced from the Continent, eagerly to adopt these new panaceas.

Thus when Rivers, Myers and others in the same position tried to use psychological treatments for psychiatric disorders in the War they were following the lead of both the Medico-Psychological Association and Freud. They were, however, swimming against the tide of professional, public and military opinion.

After the War Myers' interest in the issues subsided along with the practical needs, reviving only when the need arose, once more, in 1939. Rivers however remained fascinated with Freudian thought. He became a prominent figure in the post war discussions about the implications

of Freudian theory for psychotherapy, psychology and anthropology.

But we turn first to the "Shell Shock Controversy" of the First World War.

Myers and "Shell Shock"

Myers claims (1932) and is generally assumed to have invented the term shell shock. He coined the term to describe the psychiatric casualties which he came across among British front line troops fighting in France. The story of Myers' travels in the War and his constant attempts to become involved in applying psychology has been told in Chapter 8. Here I propose to outline his contribution to psychiatry and psychological medicine.

Myers was in a good position to conduct his work. By mid 1916, he wrote (1940 p.24), "I had myself seen upwards of two thousand cases of 'shell shock' in addition to numerous cases of other mental disorder". He came across his first cases in the Winter of 1914-1915 and wrote up a series of case studies in the Lancet. (Myers 1915, 1916, 1919.) These articles introduced his concept of shell shock to the medical world. In the first of these papers Myers emphasised that he had found fairly elementary psychotherapeutic procedures to be extremely efficacious. For example a case of functionally impaired vision, taste and smell had been cured by restoring the patient's memory through light hypnosis. In this paper Myers also pointed out that he had an open

mind about shell shock. He recalls (op. cit. p. 13).

> "I was careful to point out the 'close relations of these cases to those of hysteria'; and I did not suppose, as Lieut. Col. (the late Sir) Frederick Mott was then attempting to show, that they arose from the effects of minute cerebral haemorrhages or other microscopically visible lesions".

But he goes on,

> "I was at first by no means convinced that all cases of 'functional dissociation' arose <u>solely</u> from mental causes. The high frequency vibrations caused by an exploding shell might, it seemed to me, conceivably produce an invisably fine 'molecular' commotion in the brain which, in turn, might produce dissociation".

However as time went on and he collected more and more evidence Myers became more and more convinced of the psychological nature of shell shock <u>and</u> of the value of psychotherapeutic treatment. He tried to publish his book <u>Shell Shock in France</u> in 1916 - a book based on studies of his 2,000 cases. He intended it to assist treatment in the "various hospitals in Army Areas and also in Great Britain where cases of 'shell shock' were received for treatment". (op. cit. p. 24). However the publication was effectively censored as "General Staff was strongly opposed to the publication of articles on shell shock".

However the book finally appeared in 1940 and outlines his main achievements in detail, namely his definition of a discrete diagnostic category of shell shock and his proposals for treatment thereof.

As regards definition shell shock gave rise "to one or more of the following groups of mental symptoms, namely (i) hysteria, (ii) neurasthenia, (iii) graver temporary 'mental' disorder". In layman's language, Myers writes, this means the occurrence of various functional disorders, in the absence of physical damage such as; loss of sight, hearing, smell and touch; loss of awareness of objects; paralysis or muscle spasm; inability to perform elementary skills such as walking, speech and writing; over excitation or inhibition of the autonomic nervous system; over reaction to the senses; and lastly obsessional, often symbolic, behaviour. These symptoms usually appeared, said Myers, after the initial effects, such as purely physically caused concussion, had dissipated. However the term shell shock was, he admitted, often a misnomer - but one which derived from earlier more easily definable cases. For he writes (op. cit. p.25) all cases,

> "do not depend for their causation on the physical force (or the chemical effects) of the bursting shell. They may also occur when the soldier is remote from the exploding missile, provided that he be subject to emotional disturbance or mental strain sufficiently severe".

Thus factors such as the individual's personality, group morale, sleeplessness, cold, damp and fatigue also contributed to shell shock.

Having thus defined the condition Myers goes on to establish its psychological aetiology by arguing against the other purported explanations. No consistent physiological damage had been observed, he noted (op. cit. p.31).

Also on post mortem no consistent cellular damage had been recorded nor had a consistent pattern of disordered indices such as blood pressure, urine content, sweating, pulse rate or adrenalin secretion been observed. The sudden release of carbon monoxide and the rapid change of atmospheric pressure caused by an exploding shell were also insufficient to explain the condition. Thus he concludes

> "In the vast majority of cases the signs of 'shell shock' appear to be traceable to psychical causes, especially, in the early cases to the emotions of extreme and sudden horror or fright".

Myers advocated <u>rapid</u> treatment above all. He instructed and organised a team in France to firstly select the functional from the neurological cases. Only the most serious of the former were sent home to England. By this now obvious, but then fairly revolutionary,expedient Myers was able to cure and rehabilitate men far more rapidly than before. Hearnshaw (1964 p. 246) claims that Myers (who in his time at St. Omer worked with Gordon Holmes) managed to return the majority of the purely functional cases to the front "within two or three weeks". The nature of the treatment was fairly basic psychotherapy - but once again, at the time, it was fairly revolutionary. Myers words are eloquent enough:

> "The guiding principles of psycho-therapeutic treatment should consist in the re-education of the patient so as to restore his self-knowledge, self-confidence and self-control. For these a judicious admixture of explanation, persuasion, and sometimes scolding, is required, as in the education of children, and, where necessary, as in amnesic cases, in the restoration of a completely normal, from a dissociated, personality.

> In the milder cases of 'shell shock', unaccompanied by serious loss of memory or by severe sensory or motor troubles, the emotional disturbance may often be quickly quieted by an intimate talk, the patient being encouraged to 'confess' all his fears and worries, and induced to regard them as normal experiences in the circumstances. Care should be taken to explain to him that any mild delusions, hallucinations or other unusual mental states of which he may complain are harmless and transitory, and that they will soon disappear without danger to his future sanity. The anxiety of a patient that he will be sent to a 'lunatic asylum', or returned to the Front before he feels fit for duty there, must be suitably allayed if a speedy cure is sought". (op. cit. p 55).

Some cases were however more difficult. Fairly often the emotional disturbance would be accompanied by a loss of memory of the cause of the condition. Here, said Myers,

> "recourse must be had to the analysis and elucidation of previous conflicts or of the dreams or strange ideas which force themselves on his notice and to the revival of forgotten memories..... Such analysis and revival ..... are enormously facilitated in the hypnotic state ". (op.cit. p. 56).

Light hypnosis, claimed Myers, often succeeded when "many weeks of psycho-analytic 'free association' and 'conversation' in the waking state may fail". The procedure did this by lessening the emotional strain on the patient thus overcoming the inhibition or repression of the "buried complexes" which were the root of the trouble. The complexes, admitted Myers, could be either of recent occurrence of could relate "to long previous conflicts".

Thus Myers' approach was a broadly psychoanalytic one. He was however far too much of a pragmatist to accept more than the bare bones of Freudian theory. As we have seen he accepted the reality of repression of conversion hysteria and of long buried complexes of censored or disturbing material. He accepted the dynamic activity of the unconscious. But he did this almost entirely because his own observations supported these fairly basic claims of Freudian theory. He also noted that

> "A large number of cases, especially of those who break down merely under the stress of warfare occur in 'nervous' (psycho-neurotic) subjects who have previously suffered from mental conflicts and maladjustments, from 'fits' in childhood or from other 'nervous' attacks or breakdown".

Thus he also admitted that long forgotten conflicts could predispose patients to breakdown.

However he soon parts company with Freud. Firstly he points out that "prolonged psychoanalysis along Freudian lines "is usually completely unnecessary - as a cure can be effected far more quickly and simply by elementary psychotherapeutic measures. Secondly he regarded, with some justification, that even the most stable personalities, under intense pressure, could break down and suffer shell shock. Thus it was ludicrous to <u>always</u> delve into the deeper regions of the patient's unconscious which was, in the majority of cases, a wasted journey. As Myers himself puts it "the sexual origin of the vast majority of 'shell shock' cases is more than doubtful". Of course it is a basic tenet of Freudian orthodoxy that

psychopathological predisposition (or the potentially psychoneurotic personality) occurs through the failure to resolve predominantly sexual conflicts in early childhood.

As we have seen in Chapter 8 Myers was faced with continual military opposition to his "soft" and "liberal" ideas. Eventually through bureaucratic mismanagement (see op. cit pp 18-19) Gordon Holmes, the neurologist, through no fault of his own, was put in a position which diverted much interesting and valuable work away from Myers. This was the final straw which prompted Myers' departure to Britain and away from field work.

Back in Britain his broad conclusions were assimilated into the work going on at the Maghull Hospital, near Liverpool. Here the most serious cases were received and treated by the remarkable collection of personnel who served there. (See Chapter 8). Maghull, writes Hearnshaw (1964 p. 246),

> "had an important influence on the development of psychology and psychotherapy... it seemed pretty clear that the sexual theory of the psychoanalysts could only be a special theory not a general theory, of the aetiology of neurosis".

Rivers and Freud

Rivers' interest in shell shock, psychotherapy and Freud began with his work on the serious cases at Maghull and continued, in the latter years of the War, at Craiglockhart War Hospital, Slateford, Edinburgh. His views were at first broadly similar to those of Myers - he was however rather more fortunate than Myers in that

he was free to publish his thoughts. In 1917 in The Lancet he presented a paper on "Freud's Psychology of the Unconscious". Captain Rivers, as he then was, received a veritable deluge of letters from doctors and interested members of the public which remain in Psychological Laboratory's archives at Cambridge. Nearly all were complimentary, both on his views and on his courage in pronouncing them. A typical one came from a Dr. Leonard Blungart on July 24th 1917, who wrote;

> "Dear Doctor,
>
> I read your article on the "Freudian Theories" in the Lancet, and I cannot but express to you my admiration, not only for its excellence and its clearness, but also for the moral courage which its writing by you and its publication by The Lancet must have entailed".

Another illuminating letter on this subject arrived from Myers. Dated 20th August 1917 and from "somewhere in France" it reads;

> "My dear Rivers,
>
> I have just read your last Lancet article and write hot with enthusiasm for it. It is by far the best and most interestingly worked out case of the kind that has been published during the War..... Your conclusions agree absolutely with mine".

The letter reveals that both Rivers and Myers were agreed that supportive psychotherapy and the recovery of repressed memories were the keys to cure shell shock. This position was in marked contrast to that of the more orthodox Freudian approach of William Brown and Bernard Hart who insisted on a full "abreaction" in which the emotion as well as the fact of the traumatic experience should be

revived. Rivers and Myers on the other hand did their utmost to protect their patients from any feeling of horror or fright.

The major point of difference between the two was Myers' emphasis on the utility of light hypnosis to aid retrieval. Rivers was not so keen on this - a fact which is possibly explained by the different natures of the cases they saw.

As we have indicated, while Myers dropped his interest in psychotherapy after the War, the complications of Freudian theory became Rivers' main interest in the last four years of his life. Rivers' writings on the topic of Freud and psychology are difficult to summarise. They include papers on both very specific subjects such as "Psycho-neurotic Symptoms Associated with Miner's Nystagamus" (1922) and on wide speculation. Myers (1922) writes:

> "He had the courage to defend much of Freud's new teaching at a time when it was carelessly condemned in toto by those in authority, who were too ignorant or too incomp etent to form any just opinion of its undoubted merits and undoubted defects. He was prepared to admit the importance of the conflict of social factors with the sexual instincts in certain psychoneuroses of civil life. But in the psychoneuroses of warfare, and of occupations like mining, he believed that the conflicts were not sexual, but were the danger instincts, related to the instinct of self-preservation".

Such then were two of Rivers' contributions, firstly his part in promoting open intellectual argument about Freud and secondly he did a little to continue research into suitable therapy for the effects of industrial

events similar to shell shock. But Rivers' third and largest contribution stemmed from his attempt "to provide a biological theory for the psychoneuroses" in his 1920 book Instinct and the Unconscious.

In this book he sought to provide a physiological basis for psychoanalytic thought. "Repression" was the psychical parallel of the biologists' "inhibition" he claimed. Similarly he regarded mental illness as the failure of the later more sophisticated products of evolution to control and come to terms with older, more primitive racial tendencies. In this he was extending Hughlings Jackson's work - but he also drew heavily on his own and Head's distinction between the epicritic and protopathic nervous systems. The cruder protopathic system was, roughly, responsible for unconscious instinctive processes while the epicritic system was the physical equivalent of intelligence and reason.

Further details followed from this general approach. Conversion hysteria was for example a reversion to the primitive instinct for immobility in the face of danger. He attempted to explain all mental disorders and effects such as nightmares and fugue states in terms of the breakdown of evolutionarily sophisticated mechanisms and the regression to primitive ones.

However, Rivers received much criticism for his views both at the time and afterwards. This contribution was not a major one - but he had already played an important and courageous role in stimulating discussion on Freudian ideas.

F. A Short History of the N.I.I.P.

The Origins; 1918-1922

The history of industrial psychology in the U.K. began with two events which occurred in the 1914-1918 War. The first of these was the translation into English of Münsterburg's Psychology and Industrial Efficiency. The translation first appeared, in fact, in 1913 but became widely available during the War. The second event was the creation of the Health of Munitions Workers Committee (H.M.W.C.) Primarily H.M.W.C. was established, as Farmer (1958) put it, out of the interest in "the relation between human factors and industrial output. This was largely due to investigations carried out in the munitions factories where the pressure for higher output was so great". The success of the H.M.W.C. and the spread of the doctrine of applied psychology (popularised in Germany and the U.S.A. just before the War) eventually inspired the work of Bernard Muscio. Myers himself records (1942) that it was Muscio's book Lectures on Industrial Psychology, delivered in Sydney in 1916, and published the following year, which finally convinced him of the practicality and indeed the necessity to set up an independent institute for the furtherance of this new "Industrial Psychology".

It was in 1915 that the Government set up the H.M.W.C. to help in the twin tasks hinted at above namely; 1) to maintain a work-force who were both of good health and high morale; 2) to thereby maintain high production.

With the exception of Münsterburg almost the only precedents for this development were the now ubiquitously quoted works of Taylor (mainly on pay and incentives at Bethlem) and the Gilbreths (mainly on time and motion studies). The Taylor and Gilbreth approaches are now usually referred to as "Scientific Management." The H.M.W.C. was a body of far broader scope than its name suggests. It employed physiologists, economists and psychologists. Its work and results reflect this composition.

Farmer, for example, (ibid) was employed in movement study exercises which concerned the design of tools, work benches and so on. His main finding was, he claimed, that the simpler and more rhythmic a task could become the less fatigue and boredom would decrease production. These developments he writes were considerable advances on the previous, mainly American, studies. For as Farmer says, in nearly all Taylor's and the Gilbreths' studies changes in work patterns and/or equipment had been accompanied by changes in incentives, thus confounding the relevant variables.

The emphasis of "Scientific Management" was always, it seemed, to achieve significant increases in output. Farmer's work, on the other hand, kept payment constant and investigated ways and means of lessening the effort of work and of making it easier and more pleasant. This approach was to become fairly typical of the N.I.I.P. which quickly attracted to itself the label of the "Human Factors" school.

Myers reports (1918 p.15) two findings of the committee. For intensive work such as trench digging a group working for five minutes and then resting for ten

will soon outstrip a group continuously working. Similarly it was found that for other sorts of industrial work fifteen minutes rest per hour or a cut in the working week from 66 hours to 51 or in daily hours from 12 to 8 all brought about larger output. Staff health and morale also improved. Such were the beginnings of industrial psychology in the U.K.

After the War there was a moratorium on the future of the field. The Government's answer was to set up a direct successor of the wartime H.M.W.C. in the Industrial Fatigue Research Board orI.F.R.B. (renamed in 1929 the Industrial Health Research Board or I.H.R.B.). This body was responsible jointly to the Medical Research Council, then under the chairmanship of Sherrington and the Department of Scientific and Industrial Research. The original brief of H.M.W.C. - hours of work, environmental conditions, working methods (to include aspects of physiology, time and motion and early ergonomics) accidents, lost time and absenteeism were later expanded into more purely psychological topics such as personal reactions to work, for example monotony and psychological disorders. But although Myers (with Eric Farmer and Muscio) was one of the first members of the I.F.R.B. and, as he writes, "I worked hard to get it going, supplied pupils and vetted papers" we are not particularly concerned with this body here. In this section it is aimed to outline the contribution of the N.I.I.P. and thus how Myers' influence was exerted on British industrial psychology.

Bernard Muscio served in the Cambridge Psychological Laboratory from 1914-1916 under Bartlett's temporary Directorship. Returning to Sydney in 1916 with his head full of Taylor, Gilbreth, H.M.W.C. and perhaps most importantly the battery of simple experimental psychological techniques developed at Cambridge, he presented his Lectures on Industrial Psychology. The Cambridge expertise he had acquired included the perceptual studies of Rivers and Myers, regarding conditions governing colour vision, contrast, visual and auditory acuity work curves, the ergograph, the mental work curve and recovery from mental and muscular fatigue. Muscio was the first to attempt to describe how the techniques of the laboratory might be allied to the practical work already conducted. (Muscio 1917).

T. H. Pear at Maghull (see Chapter 8) told Myers of Muscio's book and lectures. They fired Myers with enthusiasm. He swiftly prepared his small book Present-Day Applications of Psychology with special reference to industry, education and nervous breakdown. The second part of this 47 page book is devoted to the statement of Myers' case for the psychological nature of shell shock and thus the need for its psychological diagnosis and treatment. The first part, which concerns us here, is an amalgam (coherently presented for this was a lecture delivered to Royal Institution) of the work of the Cambridge Laboratory, Gilbreth, Taylor, Muscio and Munsterburg. The book became more or less a manifesto for the N.I.I.P.

Earlier, Chapter 8 described the meeting between Welch and Myers at these lectures. In the succeeding years Welch drummed up support amongst the business community. Myers with some success at first pressed for the expansion of the work of I.F.R.B. However in 1921 with the lessons of the War dimming in people's memories, and the Treasury seeking to make economies, the I.F.R.B. was subsumed under the M.R.C. It suffered crippling financial cuts. In fact the Treasury wanted to scrap the I.F.R.B. altogether - it was only saved after considerable protest (Myers 1925(3)p 15).

However by this time the intellectual and business kudos of industrial psychology had been well established, the former by the publications cited above and the latter by the increased outputs reported by H.M.W.C. and Taylor and Gilbreth in America. As early as April 1919 Myers and Welch had instituted an organising committee for an Institute for Industrial Psychology.

As Frisby (1970) puts it,

> "The effective growth of industrial psychology depends on the acceptance it can gain from those who are the subjects of its enquiries and those who provide the material resources it must have".

Myers and Welch were clearly aware of this - and the possible implications of ignoring it. Thus the organising committee set about discussing its plans with organised labour, philanthropic institutions and the business community. The committee also appealed to "Heads of Departments in Universities and other Institutions throughout the Kingdom who were most interested in the practical application of Psychology and Physiology, for their co-operation in the work of the Institute". (Anon; Journal of the N.I.I.P. 1922).

All these approaches were successful. In 1919 promises of support were received from "Cadbury Bros., Messrs. Pascall, Messrs. Rowntree, Tootal Broadhurst Lee Co. and a number of other firms". (ibid). The Carnegie U.K. Trust also granted the N.I.I.P. £1,000 for its first five years. The initial scientific committee of the N.I.I.P. met on December 11th 1921. The N.I.I.P. had managed to enrol some 25 leading academics. They included Bartlett, Burt, Drever, Edgell, Farmer, Muscio, Pear, Spearman, Valentine and of course Myers who chaired the Scientific Committee which gave the N.I.I.P. an extraordinarily able and experienced advisory body to oversee its work throughout the ensuing years.

The organising committee of Myers, Farmer and George Miles (from psychology) and Welch, a Mr. A. F. Luke (of Whiteaway Laidlaw and Co. Ltd.) Sir R. C. Witt, C.B.E. and Mr. G. Spiller (from various businesses) had clearly laid a solid foundation for the N.I.I.P. The work of Mr. Seebohm Rowntree (of Rowntree and Co.) is also worthy of attention. It was he who organised seminars throughout the country in 1919 and 1920, usually in Universities, explaining the hopes of the N.I.I.P. Also after the Carnegie grant of £1,000 and the combined £400 from Welch, and his company Harrisons and Crosfield, Rowntree's were the largest benefactors to the N.I.I.P. (for a full list see ibid p. 222). Rowntree remained pioneers in the field of industrial psychology for many years - eventually

being possibly the first business to employ its own Industrial Psychologist. (Nigel Balchin working as an N.I.I.P. investigator for Rowntree in the 1930's designed the format for their Black Magic chocolates - which remains almost unaltered today).

The earliest organising expenses had been met by Myers, Welch, Mr. George Croll and the Bradford Dyers Association. Fuller accounts of these earliest days are available in the N.I.I.P.'s own Journal (Anon pp 2-8 1922 and Frisby pp. 35-50, 1970).

Myers was in effective control of the N.I.I.P. in its earliest years as its full time Director. Thus as we shall see its early work conforms to Myers' conception as the N.I.I.P. as a sort of industrial teaching hospital. He wrote, in 1942 "Indeed the N.I.I.P. was, in my mind to fulfil the functions of a hospital viz teaching, practice and research".

Myers had another firm view about industrial psychology. (Op. cit).

> "I came to share Pasteur's view 'Il n'y a pas les sciences appliques. Il y a les sciences et les applications des sciences'. I had to combat the view that research must be kept pure and that pure research was only worth regarding as such. I on the contrary insisted that in industrial psychology, as in medicine, research could only be helped by close association with practice".

There are two developments which follow from these views of Myers. The first was that in Britain the goal of the unified applied science conceived by Münsterburg was put aside for a good fifteen years. Hearnshaw (1942) as we shall see was to lead the later moves

towards unifying industrial psychology and re-establishing what he called its "intellectual integrity". There were other more practical consequences. If field workers were applying psychology rather than doing industrial psychology it tended to make them rely on applying tried and trusted psychological methods - instead of inventing new perspectives, especially on the social aspects of work which were badly neglected before the War. But we return to this issue later.

On the positive side Myers' emphasis on service and research (often combined) in the field led to the accumulation of an immense amount of data. The sheer volume, diversity and pioneering nature of the N.I.I.P's work before the War is astonishing - it is to that that we now turn.

It began after February 11th 1921 when the N.I.I.P. was legally established. Its constitution stated the new Institute's objectives as, "To promote and encourage the practical application of the sciences of psychology and physiology to commerce and industry by any means that may be found practicable".

The Work of the N.I.I.P. 1921 - 1939

Throughout its life the N.I.I.P. published its own Journal. This was called The Journal of the N.I.I.P. (1922-1931,) The Human Factor (1932-1937) and Occupational Psychology (1938 - to date)[1]. Much of the information below comes from the Annual Reports of the N.I.I.P. which usually appeared in the Institute's Journal. However

1 On the demise of the NIIP in 1973 Occupational Psychology was, effectively, taken over by the B.P.S. and retitled the Journal of Occupational Psychology.

from 1923-1931 these reports appeared as separate (and now very rare) publications. Some of the information also comes from various articles in the N.I.I.P.'s Jubilee Volume of 1970.

(I was able to use copies of the aforementioned Annual Reports housed in the archives of the N.I.I.P. These archives also house many still unpublished N.I.I.P. reports, minutes and correspondence. A more detailed account of the N.I.I.P. than that presented here could be written with the use of these archives.

For these Annual Reports include minute financial accounts of the N.I.I.P., a complete list of its investigators, of members, of committee members, benefactors, full time, part-time and clerical staff as well as full accounts of work undertaken - in as far as industrial secrecy permitted. A future historian of the N.I.I.P., with more time than that available to the current author, could indeed indulge in a field day in these archives. The value of these archives is increased too when one remembers that for most of its vocational guidance work the N.I.I.P. largely employed its own tests in the manner described later in this section.)

Before 1939 the N.I.I.P. was able to act mainly on two parts of Münsterburg's three-fold plan for industrial psychology. "They were," Frisby 1970, "selection of the best possible men, establishment of the best possible psychological conditions for effective work and influencing minds through advertising and salesmanship". The last aspect was that most ignored.

(i) Service

Most of the N.I.I.P.'s income came from businesses and firms which hired its investigative and hopefully ameliorating services. (The rest came from its members, interested business and individuals who paid a set fee for information and the Journals. Various private and public benefactors also contributed).

As we have seen in Chapter 8 it was one of Myers' great disappointments that the "Central Fund," which he envisaged eventually becoming available for research, never reached the size he had hoped. Nevertheless the N.I.I.P., at least in the years 1922-1934, swiftly expanded its service and advisory work.

In industry and commerce it continued the work of the H.M.W.C. and I.F.R.B. on environmental factors (light, heat humidity etc.) physiological factors (awkward postures and muscular action accelerating fatigue); the weight and design of tools; the effects of rest pauses; proper seating; payment scales, incentives, piece-work and so on. These investigations and the others mentioned below were carried out in businesses including coal mines, clothing and textile manufacturers, railways, bakers, margarine makers (all 1923) a rubber works, biscuit factory, gas works (all 1925), glass bottles, jam, perfumery and soap, motor cars, bicycles and department stores (all 1928). The list is more or less endless.

However this work in the early years was a little myopic and soon led to the development of a broader view which began to look at the larger elements in industrial

efficiency. Investigators during the early 1930's began to look at the general layout of plant and the movement of materials. The first industrial selection tests - for especially skilled operations - also began to be used at this time. But in the main this first 10 - 12 years was used to establish the credibility of the N.I.I.P. The criterion usually used was an increase in output and the methods used were those inherited from the I.F.R.B. mainly concerned as Frisby (1970) puts it "with the removal of handicaps, mainly material, which wasted the worker's energy, so that he could perform more effectively and with less fatigue".

Work study, a field connected with the above, also began in the 1920's. N.I.I.P. investigators began to stress the psychological importance of allowing personal latitude within a working method. They faced opposition in this from some quarters who maintained there was "one best way" (ibid) of doing a job. This work too led to proposals for modifying tools, methods and machines. They also advised on training programmes stressing that the learner should be reinforced constantly and given a sense of progress. Office procedures and the design of stationery and forms were improved - attempting to lessen mental fatigue for clerical workers. This work too led to a broader view which pointed out that individual satisfaction efficiency and in some cases pay depended on his task co-ordinating efficiently with the rest of the factory or office. This in turn depended on effective supplies, internal communication and transport networks.

The N.I.I.P. toyed with market research and consumer psychology in the 1930's. But as Raphael (1970) points out, despite some isolated successes (like that of Balchin mentioned earlier) it was eventually ruled outside the Institute's terms of reference.

Probably the most consistently successful work of the N.I.I.P. was in the field of vocational guidance. Its commercial service to young people began in 1922. The early service work quickly demonstrated the need for research and funds were later made available for just this. The work started on the basis of a battery of psychometric tests, a biographical questionnaire and interviews with the child, his or her parents and where possible his or her teachers. The tests, forms and interviews were constantly refined in the light of experience.

The N.I.I.P.'s approach was, it should be noted, the first major development of the "differentialist" approach to vocational guidance. The basic modus operandi was to assess or judge the personality, aptitudes, manual skills physical attributes and so on likely to make for success in a certain occupation. This ideal profile was then, hopefully, obtained from job seekers and the candidates suitability assessed. The other major approach, a largely American one, (see Super 1978) is the more individual centred "developmentalist" position.

In the early days the tests were for general intelligence, intellectual disabilities, neuroticism, special intelligence and also some aptitude tests notably one

for shorthand-typing. Quite apart from the undoubted value of the research in this field Frisby also claims that the scheme achieved a success rate of between 75 to 80 per cent. Although as he points out what constitutes occupational success was an issue, the complexities of which, the N.I.I.P.'s staff were only too aware.

Two further developments occurred in the 1930's. The first, smaller, one was a call for the investigation of inspection processes. Several companies approached the Institute expressing dissatisfaction with the quality of their own operations. N.I.I.P. workers soon went to work on the rate of work, lighting and other physical factors. More interestingly they now began to turn to the purer psychological issues. Inspectors varied, for example, in their ratings of acceptability. Raphael (1942) reviewed this work emphasising the need for a) better training and b) the availability to each inspector of items just too bad or just good enough to pass. Training techniques in other fields were also examined by the N.I.I.P. and they were especially employed in the devising of training manuals for machine operators often emphasing a carefully compiled museum of common errors.

Such then was the vast amount of industrial consultancy work performed by the N.I.I.P. It was performed by staff who were both young and inexperienced (at least at the start). It covered nearly every field of industrial activity and dealt with tasks which today employ operational researchers, ergonomists, market researchers,

industrial relations experts, time and motion experts and management consultants. Raphael (1970) writing especially about the 1920's refers to the Institute's staff - whose only qualification was usually a degree including some psychology, "They had extraordinary success - one can say 'extraordinary' considering how few of them had any previous knowledge of industry or precedents to follow".

Research

As we have seen research funds for the N.I.I.P. never reached Myers' initial expectations. Hardly any pure research was carried out on the shop floor - although the service activities just described constituted a vast reservoir of information on industrial efficiency.

However the Annual Report for 1924, joyously records,

> "Aided by the generous grant of £6,000 from the Carnegie United Kingdom Trust the Institute has therefore undertaken an investigation in a certain London area, the results of which, it is hoped, will demonstrate the great wastefulness to the nation of the present system, and the possibility of the avoidance of much unhappiness to the individual by guiding him into the employment for which he is most fitted".

The report refers to vocational guidance work of the Juvenile Employment Bureaux which had just been established and expressed the hope that research would show how the work of the J.E.B. could be improved by the utilisation of psychological tests. The good relations of the N.I.I.P. to the Ministry of Labour and the London County Council are also mentioned.

The main result of this was a long-term study of the effect of vocational guidance on 1,200 school leavers in King's Cross, London. This gave the N.I.I.P. the chance to develop and assess new tests as well as (for the first time) the chance to select its own sample. Before this, vocational guidance had been conducted only for those whose parents could afford it. Thus it had been a largely middle class, well educated sample.

In 1926 things became even better when a Rockefeller grant of £2,000 per annum was made available for the next five years for general research purposes. Some of this supplemented the vocational guidance research. Described by Frisby (1970 p. 42) these funds enabled research to be undertaken, in rural versus urban areas, in borstal institutions, on test re-test reliability (in Fife), and for blind persons. In May 1927 a further £2,500 was made available by the Carnegie Trust to ensure an efficient follow-up of the King's Cross experiment. Also in 1927 an exercise in occupational analysis was undertaken with especial reference to nurses, teachers and secretaries. Tests were being developed for Mechanical Ability, Manual Dexterity and the Distribution of Attention.

Turning to tests, Frisby notes that tests for specific occupations were an early priority of the Institute. This was mainly because the Institute's vocational guidance service was a reliable and substantial source of income. Early tests included those for "clerical work, engineering, shorthand-typing, dress-making and tailoring."

Work on tests of general and special aptitudes followed almost immediately and remained a regular preoccupation of the Institute. Other vocational research included that by Oldfield on interviews and by Miles and Vincent for motor driving.

Some smaller grants were made available for research in working situations. In retrospect those of Harding (later Professor at Bedford College, London) on the psychological importance of the unit of work were among the most important. This development - a new concern for work as the worker saw it was pursued by Raphael in 1937. She looked at causes of unrest, dissatisfaction and high labour turnover, making extensive use of interview techniques.

Staff and Finance - A Miscellany for the N.I.I.P. 1921 - 1932.

In 1924 the fees received for the Institute's general services increased by 49% to £5,514. This was followed by yearly increases of 10%, 28%, 37% and 29% reaching £16,400 in 1929. This pattern with a parallel increase in membership continued into the early 1930's which were probably the N.I.I.P.'s peak years. Demand for vocational guidance stood at 80 per annum in 1923, reached over 300 in 1928 while it often topped 1,000 in the 1930's under Alec Rodger's direction.

The Annual Report for 1932 recorded the N.I.I.P.'s "biggest year in all respects". In fact until the mid 1930's demand generally outstripped capacity for all

the Institute's services. Until 1932 the financial picture of the N.I.I.P. is simple - it was one of a steady growth which was inspired by its industrial successes. The charitable grants, which were attracted by the Institute's academic respectability, provided sufficient funds for some important research.

The N.I.I.P., especially in the 1930's, had extraordinary success in attracting both able personnel for its investigative staff and respected public figures for figurehead offices such as president - the Earl of Balfour and Viscount D'Abernon being the first two.

From the beginning some of the country's best psychologists served on the Scientific Committee. The highest point of the N.I.I.P. hierachy was its Council. This was a body bursting at its seams with psychologists such as Drever, Pear, Spearman, Thomson, Edgell, Bartlett and Watt. It was also full of businessmen such as Seebohm Rowntree and E. R. Debenham. The N.I.I.P.always maintained a high standard of both business acumen and academic experience on its higher bodies. This was eventually to lead to disputes between the academic and business interests - but we deal with these later.

Perhaps an indication of the respect the N.I.I.P. rapidly gained was given when the Duke of York, the Prime Minister and Viscount D'Abernon all spoke on behalf of the Institute at a dinner held in 1929. The N.I.I.P. was also never slow to blow its own trumpet. Writing

of its Appeal Fund in its Annual Report for this year it claims

> "The Council has no hesitation in urging those industrial companies and individuals who have the support of the Fund under consideration and those who have not yet considered the matter to help the Institute in this work which is so important for the economic and social life of the nation".

However the Prime Minister, then Ramsay MacDonald, had already spoken thus,

> "The great contribution, that this Institute is making to our industrial efficiency is, I venture to say, this - that it is constantly keeping in front of us the fact that prosperity and progress must be measured by human advancement, moral advancement, aesthetic advancement, spiritual advancement, as well as by material advancement..... If you were endowed with something like a million a year you could spend it in a way that not a farthing of your endowment would be wasted".

In fact Government support for the N.I.I.P. was as good as non-existent. The depression and the succession of Conservative governments of the 1930's staved off possible improvement of this situation and it took the N.I.I.P.'s massive contribution to World War II and a government enquiry to finally prise money from the Treasury.

The full time staff of N.I.I.P. in 1922 numbered eight. Myers was director with Miles as his assistant and secretary. Cyril Burt, at this time employed as the L.C.C.'s first psychologist, for two years divided his time equally between N.I.I.P. and L.C.C. He headed the vocational guidance department until 1924 when F. M. Earle resigned

a professorship in education in South Africa to succeed him. The vocational guidance section was the Institute's strongest point perhaps. In 1931 it was headed by Angus Macrae and included Mary Stott and Alec Rodger who went on to lead the section. By 1925 Miles had been released from full time secretarial duties to become Myers' full time Assistant Director. In 1930 Myers became Principal, to devote more energy to publicity and fund-raising to bring about his aim of amply-funded research. Miles became Director. By this time a Scottish Office had been set up; there were 39 investigative staff and another 12 clerks and laboratory staff; the offices at 329 High Holborn, despite three enlargements had still proved too small necessitating a move to Aldwych House; membership had risen from 100 in 1929 to 650 in 1925 to a level around the 1,500 mark in the early 1930's.

The Teaching of Industrial Psychology

Myers had an uncanny knack of being in at the beginning of things. He had instituted psychology as an examinable subject at Cambridge in the pure, clinical and anthropological fields. In 1922 the N.I.I.P.Annual Report records that

> "Dr. Myers and Professor Spearman submitted recommendations from the Institute to the Board of Psychological Studies of London University with a view to the establishment of a Diploma in Industrial Psychology. The institution of an Academic Diploma in the subject has since been approved by the Senate of the University".

The second such Diploma was set up by the N.I.I.P. in Glasgow following the institution of its Scottish section in 1930. These Diplomas involved N.I.I.P. staff in a large amount of teaching on courses which the Annual Reports record were always well attended.

But over and above this the N.I.I.P. was attempting to catch public attention - as well as that of businessmen and trade unions. From the very start radio broadcasts were made. In 1923 "Short talks on industrial psychology were broadcasted during the year from the London Station by Dr. C. Burt, Professor E. E. Collis, Mr. E. Farmer, Dr. G. H. Miles, Dr. C. S. Myers, Mr. B. S. Rowntree and Mr. H. J. Welch". A similar pattern was retained throughout the pre-war period.

Myers' services became increasingly in demand as a visiting lecturer to various bodies. In 1923 a course run jointly by N.I.I.P. and I.F.R.B. began at the L.S.E. while in the same year demonstrations and/or talks were given to the Royal Society, the British Association for the Advancement of Science, the British Medical Association and two government committees attached to the Board of Education. In later years the L.S.E. courses continued while lectures were delivered literally all over the country to various Trades Unions, Professional Institutes, Rotary Clubs, Advertising Associations, Universities and other bodies in any way connected to industrial matters.

Thus until the early 1930's the N.I.I.P. enjoyed a thoroughly successful infancy. Its adolescence was to prove a little more painful.

The Troubled 1930's

Alec Rodger was head of the N.I.I.P.'s Vocational Guidance Section for the immediate pre-war years, having joined the Institute in 1929 he discussed the problems of this time in his C. S. Myers' Lecture (Rodger 1971). He writes:-

> "In the first place the industrial depression of the 1930's had produced unexpected difficulties. The Institute, though 'a scientific association not for profit', was nevertheless at that time dependent for most of its income on fees earned. Occasionally it had had, in the early years, research grants from the Rockefeller, Carnegie and other trusts; and there was a small but useful flow of subscriptions and donations. But these had always been hard to win, and in the thirties they became harder. On Myers himself fell the main burden of money raising, and it was one that he found both difficult and distasteful. On the so-called 'industrial investigations' side of the work, earnings fell from over £20,000 in 1930 to less than half of that some years later. A second problem lay in the deterioration of relations between the Institute on the one hand and the Medical Research Council (and its Industrial Health Research Board) on the other".

In Chapter 8 Myers' own view of this was discussed - it was a source of great irritation and disappointment to him. Rodger (1977) has confirmed that the publicly expressed goodwill and co-operation between the bodies, usually in their annual reports was "a facade". But even this formality became less well observed in the late 1930's.

Rodger continues:

> "A third problem arose from what Myers regarded as the obstinacy of the businessmen on his executive committee. In spite

> of the fact that he himself had a business background he disagreed with some of them - especially with Welch and Robert Witt - frequently. He was not commercially-minded enough for them".

Rodger has ably outlined the three problems which dogged the N.I.I.P. throughout the 1930's - finance, external relations, and internal conflicts. The euphoria of 1932 was followed by a succession of rather gloomy Annual Reports. That of 1934 tells us that research was having to be curtailed through lack of funds. This worrying, if not terminal, condition continued, alleviated slightly by a major appeal for funds in 1936, until the War. Undaunted, the Institute battled on maintaining "all its major activities" (Annual Report 1939). These were of course the fund raising ones of industrial consultancy and vocational guidance. Rodger in 1938 for example reported testing 1,393 individuals, as well as undertaking work for the West Sussex Education Committee and running a course for intending careers advisors at the L.S.E.

The financial tightrope walked by the N.I.I.P. at this time is amply illustrated by the Annual Report for 1938. The success of the 1936 appeal, it records, made possible the opening of a Manchester office. Yet by Spring 1938 the situation was again "critical" and "it was compelled therefore to embark on a scheme of drastic retrenchment and this has been in operation during the last six months". The "retrenchment" involved, redundancies, harder work for the remaining staff,

reduced salaries and the winding up of a staff superannuation fund.

Hardly surprisingly the staff took a dim view of this. Rodger (1971) records how Miles, then Director, and Myers, then Principal were often at loggerheads over staff productivity. There were constant threats of resignation. At one time Rodger and some colleagues went so far as to look for suitable premises for their own Institute - for which they wanted Myers as Director.

To slightly oversimplify this complex situation, financial stringency had brought to the surface the latent conflict between the businessmen and academics in the N.I.I.P. The business faction saw little wrong in the Institute's first ten years of work which had produced economic returns for its clients. As Hearnshaw (1964) puts it, ".... because industrialists wanted returns, there was a temptation to fall back on a line of country that promised fairly easy and quick productive increases".

However, and this is a fourth point of unrest which I would add to Rodger's three, industrial psychology was becoming a far more academically respectable area. In 1933 Elton Mayo had published his Human Problems of an Industrial Civilisation. This book clearly pointed out the need for larger scale, longer term research in the industrial field. It especially stressed the problems of industrial unrest, worker dissatisfaction and their links to social factors. If Myers' research fund had reached his expectations the N.I.I.P. might have been

able to meet this new challenge. As it happened, just as this message was getting across to the Institute's staff (viz the publications of Harding, 1939, Raphael 1937, Bevington 1937,) the research funds were reaching their lowest ebb.

Academically this conflict finds its best statement in Hearnshaw's (1942) article "The Unity of Industrial Psychology". Here Hearnshaw emphasised that industrial psychology was not merely the applications of psychology to industrial problems. It was he claimed a field of its own, with its own methods and subject matter. Recognition of this, led, he claimed, to a need for: more research in industrial psychology; training in the subject for future practitioners - which would lead to more well founded field work; an increase in scope to include social factors.(1)

Such men as Rex Knight, D. W. Harding, Alec Rodger and Hearnshaw himself (all later Professors of Psychology) could hardly have been unaware of the needs of industrial psychology in the 1930's and thus of the limitations of the N.I.I.P.

However the ferment had somewhat subsided by 1939. Clifford Frisby had taken over as Director in 1938 and had taken a firm grasp on the administrative side. Myers, whose grasp of the day to day details of running his Institute had never been his forte was elevated to Honorary Scientific Advisor. The Annual Report for 1939 records a "slight financial recovery". Reduced salaries were

(1) As Hearnshaw himself later pointed out (1964 p. 282) expirical and applied social psychology in Britain only developed in the late 1930's after escaping "the shackles of McDougall's instinct theory and academic sociology." See also Chapter 14.

still in operation, few new developments could be planned. However as we now know such considerations were soon to be made rather academic by the outbreak of War.

The Achievement of the N.I.I.P. 1921 - 1939

The achievement of Myers' Institute is fairly simply stated. The N.I.I.P. pioneered work across the whole spectrum of industrial and occupational psychology with the exception of consumer and market research. For the later pre-war years much of this field was also covered by the Government's I.H.R.B. But, as we have noted, Treasury support for industrial psychology was far from enthusiastic in the early 1920's. The successes of the N.I.I.P. probably did much to improve the reputation of industrial psychology and thus indirectly probably did much to attract funds to its sister, and rival, body the I.H.R.B. Nevertheless the initiative taken by Welch and Myers is the largest single reason why as Boring puts it:

> "applied psychology caught on in Great Britain more rapidly, as compared with the development of experimental psychology, than it did in America". (p. 488, 1950).

This process was indeed so marked that it caused Boring (p. 493) to further remark that, "Applied psychology, finding support in England in the post-war period soon outstripped its academic parent".

For the N.I.I.P. made significant strides right across the range of its work; in devising reliable and valid tests for aptitudes, skills and vocations - providing the knowledge and personnel for a large proportion of

the military selection work in World War II; in devising research methodology; in 'selling' industrial psychology to businesses, trades unions and universities; in ergonomics and latterly in industrial relations. In fact it built the foundations on which much, and arguably most of current British industrial psychology is built. Certainly it provided an important training school for many of Britain's leading occupational psychologists of the post-war era.

Perhaps the most important single point to be made is that the N.I.I.P., from the very start, offered a counterpoise to the rather exploitative and mechanistic view of industrial psychology so widely popularised by Taylor. Sofer (1972) names four advantages of the N.I.I.P.'s approach over that of Taylor. Firstly they were far more aware of individual differences. This facet was best illustrated by their "differentialist" approach to vocational guidance. Secondly they recognised the full complexity of human motivation - and desisted from applying any simplistic economic model. Thirdly they tended to view the worker as an integral part of a work system or organisation - and not purely as a rather isolated individual. Lastly, Myers characteristically sought to satisfy the demands of both sides of industry. The following passage from Myers 1925 publication Industrial Psychology in Great Britain perhaps serves to illustrate the point (p. 26, 27, 28).

> "When the N.I.I.P. was being established, it was obvious that the workers were straight away prejudiced against it by

> such terms as 'efficiency' and 'scientific management'..... they feared speeding-up and the dismissal of their less competent comrades. The mention of scientific management made them suspect that they would be degraded to the position of servile mechanisms. Taylor's endeavour to establish 'rigid rules for each motion in every man,' followed by Gilbreth's 'Quest of The One Best Way' is in diametrical opposition to the attitude of British workmen".

Myers then goes on to point out that all individuals have their own distinctive way of doing things and that this should be recognised. Initiative should also be encouraged he argued - totally against Taylor and Gilbreth. Myers ends by writing,

> "The N.I.I.P. has endeavoured to base its ideals on sound psychology rather than on the superficial analogy with a piece of engineering mechanism. It has sought not to press the worker from behind, but to ease the difficulties which may confront him.....by such a procedure it has gained the confidence of the worker".

While this may not have been totally the case Myers' approach was certainly a vast improvement on its American counterparts.

However as the years have gone by some of the limitations of Myers' approach have begun to emerge. The first which we have mentioned earlier is that pointed out by Elton Mayo and his Harvard colleagues. This was of course that the N.I.I.P. tended to undervalue the importance of social values and norms which would inevitably come into play in the work situation. Mayo was also critical of Myers' approach to industrial conflict. Mayo regarded employee employer conflict as inevitable

while Myers regarded it as something of an irritant which would disappear given more efficient management. A more recent but allied criticism is that of Argyris. As Sofer puts it;

> "Argyris has pointed out that the traditional approach of industrial psychology was to assume that the organisation is in a steady state and is a constant that can be taken for granted, while the investigator examines particular tasks, layouts, and persons. Insofar as the organisation is in fact in a steady state this, of course, by-passes the important question of what is keeping it in such a condition. But, more likely, the organisation is not in a steady state, and this is worth understanding from both the scientific and the practical point of view.
>
> Argyris has made the further interesting suggestion that the assumptions of the industrial psychologist, his acceptance of the brief put to him, have the effect of reinforcing the status quo. This is because the tasks given to industrial psychologists typically involve trying to make the existing organisation work better, cutting down the immediate causes of friction, and reducing control problems by helping in the selection and training of people to fit the structure better".

These criticisms are, as Sofer points out, still pertinent today - so perhaps we should not be too critical of the N.I.I.P. for committing these errors over forty years ago!

The Institute, as a private, non-profit making body, dedicated to industrial psychology was without precedent. As far as this author is aware it is also without parallel or sequel. Yet without it industrial psychology in Britain would have been subject to the whims of government -

and would certainly have suffered gravely as a result.

After the War the N.I.I.P. was reconstituted and concentrated more on pure research. In 1973 it went into a state of suspended animation through financial difficulties. A state in which it remains today.

Its current situation is rather a sad state for Welch and Myers' Institute to be in - the N.I.I.P. was a brave and pioneering venture which has a proud and adventurous history.

G. Rivers and Myers; A Final Assessment

The Psychological Foundations of the Cambridge School

By 1922 Rivers and Myers had established a thriving programme of psychological research at Cambridge. The work of the Cambridge School was firmly under way and its eclectic, practical and broadly based experimental character had been fully formed.

The final character of the Cambridge School came about through a thorough mixture of many and diverse factors. Firstly Rivers had imported the physiological psychology of Germany, most notably that in the visual field of Hering and Helmholtz. By his own researches and his article for Schafer's Text Book he firmly established British experimental studies of vision. Then came the Torres Straits expedition which emphasised the utility of the psychophysical methods so long advocated by James Ward and the intrinsic interest of cross cultural observations. Myers then came along to prosecute

experimental studies of audition for the first time in Britain while Rivers performed innovating work on fatigue. By 1909, when Myers' Text Book appeared, importing the majority of German work of any interest, Cambridge psychology embraced Pavlovian animal studies, experimental aesthetics as well as applied work for teachers. The Cambridge School took on its firmly applied character during the War when Rivers, Myers and Bartlett all showed that psychology could be useful both to the medical profession and to the military. The War also provoked, indirectly, Myers' conversion to industrial psychology and thus the establishment of the N.I.I.P.

Thus, under Rivers and Myers, Cambridge Psychology embraced among other things British Systematic Psychology, Fechner's Psychophysics, Wundtian Structuralism, physiological studies of the senses, Cross Cultural Psychology, Freud's psychodynamics, Pavlovian reflexology as well as applied studies in Education, Medicine and the armed forces. Yet the Cambridge School adhered firmly to not one of these schemes. Why was it that during the years that gave birth to the fervent schools of Behaviourism, Structuralism, Gestaltism and Psychometrics the Cambridge School steered a course, carefully, between all of them?

The current author has reached the conclusion that there were two main reasons. The first is the continuing influence of the philosophy and psychology of James Ward and the second lies in the personalities of Rivers and Myers.

The Continuing Influence of James Ward

The two main "schools" of this era were of course Wundt's Structuralism and Watson's Behaviourism. As we have seen above (Chapters 1 and 2) and as Mackenzie (1974) and Ben David and Collins (1966) have pointed out, Wundtian Psychology enrolled the philosophical support of British Associationism. The work of the Mills and Alexander Bain proved useful in the formulation of a scientific psychology. Germany at this time (the 1870's) was in the grip of a scientific materialism which had arisen for various reasons. Among these were the manifest achievements of the natural sciences and the demise of the German Idealist tradition. Wundtian Psychology thus fitted social and cultural trends.

Similarly Watsonian Behaviourism fulfilled, or rather claimed to fulfil, social needs. It also had the philosophical support of the predominantly American schools of Operationism, Pragmatism and Functionalism.

No such situation occurred in Britain, where, as we have noted, the predominantly materialistic Associationist tradition was being met head on by the imported German Idealism. At the extremes Bain was proclaiming a purely mechanistic materialistic psychology while Bradley and Green were arguing that such a thing was totally absurd mainly on the grounds that consciousness was, a priori not capable of analysis. Or as Bradley 1887 put it consciousness is "a continuous mass of presentation in which the separation of a single element from all context is never observed".

James Ward's achievement was to steer a middle course through this potential philosophical quagmire. As we have recorded in Chapter 5 Ward saw a place for a purely experimental psychophysics and for a more teleological or humanistic approach. Ward tried to promote the experimental study of the senses while, at the same time, promoting interest in the nature of individuals' experience. Ward of course was not entirely successful - but this is not in the least surprising as the debate between the mechanistic and humanistic factions in psychology still continues today and is still thoroughly unresolved (see for example Allport 1955, Shotter 1975 and Joynson 1974). But what Ward did achieve was to point to these two, possibly irreconcilable, realms of human action - the mechanistic and the purposive. Thus he indicated the wide ranging requirements for a psychology.

Rivers, rather surprisingly, never explicity recorded his views on the nature of psychology. Fortunately Myers did. His philosophical standpoint was illustrated in two papers. "On the Nature of Mind" was his 1931 Presidential Address to the Psychological Section of the British Association and "The Absurdity of any Mind-Body Relation" was Myers' 1932 Hobhouse Memorial Lecture. The abstract of the latter reads:

> "All that we can surmise about the ultimate nature of lifeless matter is that it is mechanically active. The essence of matter consists in process, not in substance; its esse is not percipi, but agere: its qualities and properties are known to us

only by our conscious experience of them - that is to say, by the interaction of its own mechanical activities with our own self-activity. We are bound to admit the existence of mechanical (and other) activities, independent of our own self-activity.

Living matter is characterised by an inherent activity totally different from that alone inherent in lifeless matter - an activity not blindly mechanical nor involving the expenditure of mechanical energy, viz. directive activity. The system of the whole universe also exhibits, in its history and organisation, a diffuse directive activity; but in the living organism directive activity is inherent within the system of the individual manifesting itself, e.g. in the individual's struggle for existence. With the specialisation of functions and differentiation of tissues which occur in the evolution of animal life, this directive activity, persistent throughout every tissue, becomes most highly developed in the nervous system. The various higher and lower directive activities within a living organism are never in perfect harmony; further, they are controlled by, as they themselves guide, purely mechanical activities.

Throughout life, directive activity, together with the associated, characteristic, mechanical activities, is synonymous with mental activity. The activities of what we artificially separate as living matter and mind are identical, each comprising the same directive and peculiar mechanical activities. There can be no mind-body relation in the presence of this identity. The only distinction that can be drawn is one between the living individual and individual objects of the lifeless universe.

The highest directive activity in the living organism is that of the 'self', which becomes less uniformly diffuse and more pontifical, as, with the evolution of the nervous system, directive activity becomes more and more highly distilled and sublimated in the evolving self. All conscious mental activity is self-activity;"

Essentially Myers is saying the fundamental fact of the universe is its activity - because our knowledge of

of everything is based upon activity. Matter exhibits only mechanical activity but when matter becomes sufficiently organised mental and purposive activity begins to occur. Myers' lecture was essentially an up-dated version of Ward's panpsychism. He does however make plainer the view implicit in Ward that "mind and life are identical properties of what we term living matter". Myers hoped, by taking this stance, to solve the traditional problems of the mind-body question in a similar way to the Identity Theorists did after the Second World War. That is he hoped to sidestep the problem of interaction and relation which had dogged psychophysical parallelism and dualism, while leaving a sufficient place for mind and consciousness which monism or epiphenomenalism had failed to do.

The importance of these views was illustrated by Myers in his "Aspects of Modern Psychology" (Myers 1941). In this paper Myers reviews the modern history of psychology criticising as he goes along the various schools which have arisen. The paper is a marvellously comprehensive statement of the assets and defects of the various psychological schools. He writes:

> "Neither Wundt nor Titchener, with their accent on introspection, took any real interest in individual mental differences... Animal and child psychology did not interest them; nor could they find room for abnormal and applied psychology".

Of Freud's followers he writes:

> "Loose terminology, needless anthropomorphisms and ridiculously wild generalisations have, from the scientific standpoint,

been the ruin of these men of genius".

Over emphasis on the private world, by not only Wundt but also by Spranger in Germany, Allport in America and McDougall in Britain led to a revolt and to Behaviourism, writes Myers. This position he writes;

> "is forced to maintain the two absurd standpoints that consciousness is of no biological, functional, significance whatever in human and animal life; and that the highest, noblest mental responses and personality itself are nothing more than the mechanical integration of the lowest and simplest reflexes..... It is possible, but, as I have urged it is insufficient, to regard psychology as a science in which the "private", personal, nature of mental experience is transformed into arrays of symbols which have been derived from "publicly" observable events, i.e. behaviour".

Myers' solution to the various problems of the various schools is as follows:

> "Wherever we turn, whatever methods we consider, it would seem that both the total wholes and the component parts require appropriate study in order to arrive at a true and complete psychology. Once again we are forced to the conclusion that psychology needs to be studied not only from the mathematical and mechanistic but also from the humanistic and teleological standpoints, and alike from the introspective, behaviouristic and Gestalt standpoints, according to the purpose which the study is intended to serve and the conditions under which the study is undertaken. We may justly complain that any simple current concept, e.g. that of the reflex, of association, of Gestalt, or of factors, is inadequate, and that broader or multiple concepts are desirable".

Myers clearly embraced Ward's philosophy. His recognition of the need for study of the mechanistic and the teleological aspects of human behaviour stems from Ward's thinking. But the execution of these aims was almost entirely due to the down to earth and practical approach of Myers himself.

The Characters of Rivers and Myers

As we have seen in Chapters 8 and 9 Rivers and Myers were both medical doctors. By training they were thus encouraged to bring whatever knowledge they could, to bear on practical problems. Both dabbled with medical careers and then with anthropology. Rivers was something of a polymath who found it difficult enough to adhere to one discipline - let alone to one school within a discipline. Myers was a fiercely practical man who devoted most of his life, in the end, to applied psychology - which he saw as similar in some methodological respects to medicine. Both men were thus by training - as well as by temperament - opposed to dogma. Both men had wide and practically inclined interests.

There has been no single school of psychology, before or since, which would have satisfied the scope which Rivers and Myers saw for psychology. That was why they never embraced a single school - it was also why they never formally attempted to found one. But effectively they did, because Bartlett, after them, saw the value of their

eclectic practical approach. He passed the message on to pupils who afterwards were to play a leading role in British psychology as we shall see in Part 4.

Summary

The contribution of Rivers and Myers can perhaps be summed up thus:-

Together they:-

1) Showed the intrinsic and anthropological value of cross cultural psychology by their investigations on the Torres Straits expedition.
2) Demonstrated the psychological nature of shell shock in World War I. With the other medical psychologists involved they succeeded in humanising treatment and afterwards this helped to achieve the recognition of clinical psychology.
3) Imported and translated important early German experimental work. Rivers' article on vision in 1898 and Myers Text Book in 1909 between them made much important German work available for the first time in Britain.

Rivers alone:-

1) Began experimental studies of fatigue, drugs, colour vision and visual illusions in Britain thus starting off fruitful research areas.
2) Made important contributions to anthropology especially in methodology. He dismissed Bastian's evolutionary theory and put new emphasis on the social psychological study of culture rather than on material objects.

3) Was an early publiciser and debater of Freudian concepts especially in their relation to psychology, psychotherapy and ethnology.

Myers alone:-

1) Began British studies on auditory localisation - which led to practical applications for submarine detection in World War I and Banister's important work afterwards.
2) Played the leading part in organising (and paying for) the new Cambridge laboratory in 1912.
3) Was mainly responsible for instituting a wide ranging research programme at Cambridge incorporating Pavlovian and German initiatives as well as more original ideas such as experimental aesthetics and educational studies.
4) Formed the N.I.I.P. in 1921 and thus pioneered the whole spectrum of industrial and occupational psychology in Britain - with the exception of market research.

PART FOUR

F. C. BARTLETT:

The Consolidation and Growth of Cambridge Psychology 1922 - 1939

CHAPTER 11

INTRODUCTION

The period 1922 - 1939 saw fundamental changes occur both in the substantive character and in the popular acceptance of British psychology. The promise of applied psychological research suggested by the work in World War I had much to do with this. After the War, Bartlett wrote (1927(3))"the psychologist is no longer wrapped up in himself..... The psychologist must now go out into the world of daily events".

This new willingness to tackle practical, real life problems bore fruit, as we have seen in Part 3, in the N.I.I.P. and the government's I.H.R.B. The fact that psychology had shown itself to be useful also profoundly affected the growth of Cambridge psychology. Considerable funds were soon attracted to the laboratory for applied research. This process was aided by Bartlett's close contact with Myers, by his own growing influence on government committees and by his great personal interest in applied psychology. In the immediate pre-war years Eric Farmer and Eric Chambers were lured to Cambridge posts from the I.H.R.B.. Indeed the late 1930's saw industrial and applied research occupy pride of place in the Cambridge laboratory. The Medical Research Council provided most of the financial wherewithal.

The eventual results of this process were two fold. Firstly, when the Second World War came, Cambridge was

the natural place for much applied research to be conducted. Secondly, when the War had ended, and psychology had yet again shown its utility, the M.R.C. decided to set up an Applied Psychology Unit at Cambridge. The A.P.U. under its successive directors, Bartlett, Mackworth, Broadbent and now Baddeley has, over the years, established a world wide reputation for itself. But it owes its very existence to the advocacy of Bartlett during the 1930's. This growth of applied psychology at Cambridge is described in Chapter 13.

Cambridge psychology also matured and grew in a purely academic sense during this time. Bartlett's laboratory attracted ever increasing numbers of pupils. The courses offered to them grew steadily in number and variety. The old philosophical approach gradually disappeared, being replaced by more lectures on the physiological, the industrial, the educational, the clinical and the cognitive aspects of psychology. Indeed the whole approach of Cambridge psychology became more modern. Perhaps the only glaring omissions, when compared with a contemporary psychology course, were social psychology and personality, while the physiological was also underplayed by modern standards.

For Bartlett himself these seventeen years saw him secure his place in psychology's history. He produced his major work Remembering in 1932. It is a book which virtually every psychology student since that date has,

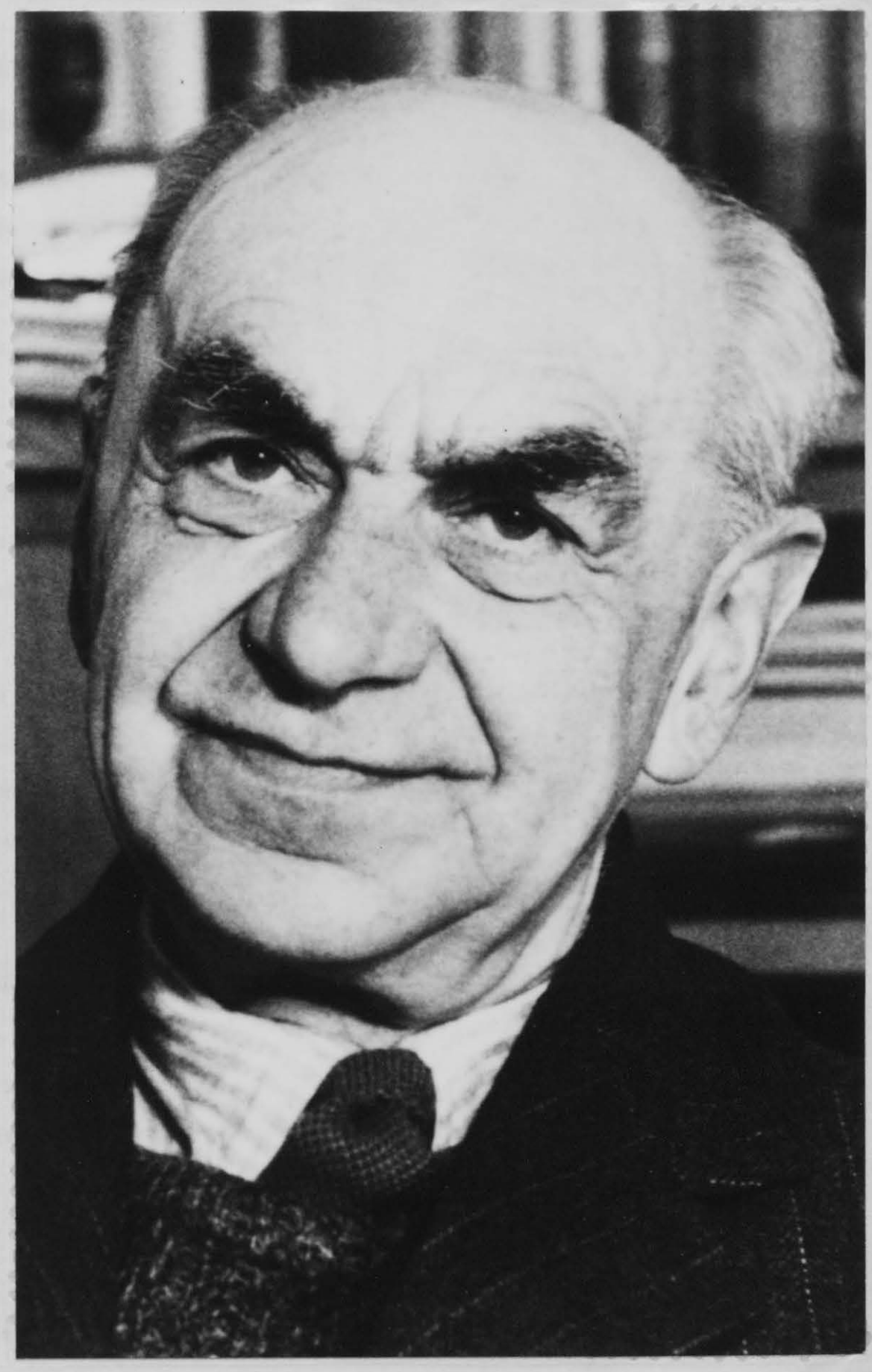
at least, been made aware of. It also secured him a world wide reputation. This period also saw Bartlett build up a famously successful teaching department. No less than thirteen future professors of psychology were trained by Bartlett in these seventeen years. The third aspect of Bartlett's progress in this period has been mentioned above and was, of course, his growing influence on government committees concerned with applied psychology.

During these inter-war years British psychology in general came of age. Chairs were established at three London colleges, Manchester, Edinburgh and Cambridge. Departments sprang up in Aberdeen, Glasgow, St. Andrews, Liverpool, Reading and Oxford. Meanwhile Spearman, Bartlett, Myers and McDougall were all Fellows of the Royal Society. Applied psychology became firmly institutionalised, though not without teething troubles, and became accepted in the fields of education, the armed forces, mental illness and of course industry. Cambridge and Bartlett played an important role in most of these developments and it is this role which we attempt to describe below.

PLATE 11

Sir Frederic Charles Bartlett 1886 - 1969

"I myself feel that some of Bartlett's insights have still not had their full impact and that they will come into their own in the next generation. But even if this is not so, those ideas which are already fully appreciated have a secure place, and are unlikely to be seriously challenged". D. E. Broadbent 1970(2).



CHAPTER 12

SIR FREDERIC CHARLES BARTLETT - A BIOGRAPHY OF HIS EARLY YEARS. 1886 - 1922.

(All quotations in this Chapter, unless otherwise attributed, are from Bartlett's unpublished autobiography "What's the Use of Psychology").

A. From Stow on the Wold to Cambridge.

Frederic Charles Bartlett was born on October 20th 1886 in Stow on the Wold. His father, William, ran "a shop for the manufacture and sale of boots and shoes", which was "reasonably flourishing". His maternal grandfather owned a slate quarry. In fact, many of his mother's family were employed in local building and farming. In his latter years Bartlett developed some interest in his family history. He came to the tentative conclusion that the Bartletts were a traditional, well establishd Cotswold family. He even found a 14th century Bartlett, buried in nearby Saintbury graveyard.

He writes "The first fourteen years of my life I spent as a normal country boy, largely out of doors". He remembers helping with the harvest, playing village cricket and even seeing W. G. Grace play at Cheltenham. This latter event he recalled, "must have played a part in developing a love of cricket which has persisted all my life long".

Bartlett had only an elder brother and a premature, retarded younger brother. His father could thus afford

to secure a place in a local private school for his second son. This school was "pretty good," Bartlett remembered, but although English was taught well, "mathematics teaching was nowhere as good and this I have since regretted". In fact Bartlett, who only started school at the age of 14, was soon invalided out of what little conventional schooling remained for him. He had a severe attack of pleurisy in November 1901. "This illness," he writes, "and the following prolonged convalescence were turning points in my life" - for they led to his interest in psychology.

The illness left Bartlett too weak to attend boarding school and his father determined upon private tuition. Soon even this plan went awry when, as Bartlett puts it, "an earlier ambition, which in truth I had never given up, to write stories and novels, returned in force". His father, as ever, was encouraging and a typewriter was purchased to aid the venture. The results were "reasonably good" but when Bartlett was seventeen his father persuaded him to read for an external London degree through the University Correspondence College whose headquarters were at Cambridge. Thus, at the advanced age of seventeen, Bartlett finally started on his education proper. Oldfield (1972) describes the time like this;

> "He was left to educate himself supported by his father's mental vigour and large assortment of books, by the stimulus, encouragement and library of the local non-conformist minister".

Not surprisingly, at first Bartlett found it "very hard going". His first exams were a "magnificent failure" - in all but English and Logic. In all Bartlett was studying five subjects. His decision to take Logic had been spurred by a friend of his father who said, "Well why not take Logic, you might like it". "It seems odd," Bartlett continues, "that this suggestion, made, as it appeared, very much on the spur of the moment, should have settled pretty well all of my future; but so it turned out".

Bartlett passed his preliminary exams at the second attempt and his intermediate exams at the third. By this time his health was improving. Helped by "several local people" he worked "long and hard" and resumed his cricketing career. "By now," he writes, "I was thoroughly interested in Logic and had begun to read widely in related subjects".

This reading, luckily for psychology, included Stout's Manual, Groundwork, and Analytical Psychology. He also recalls, (1936) cycling, once a week, the eighteen miles to the nearest public library to read James Ward's famous Article in the Encyclopaedia Brittanica.

Towards the end of his degree Bartlett was beginning to make up for lost time. Things became progressively easier for him and he passed his B.A. in 1909 with First Class Honours. His major subjects were Logic and Philosophy. The problem of what to do next was soon solved. The Principal of the Correspondence College invited

Bartlett to come to Cambridge to become a tutor. "I accepted without hesitation," Bartlett recalls, "and on a misty morning in the Autumn of 1909" he left Stow.

The next two years were thoroughly enjoyable. Bartlett enjoyed his work, played a lot of cricket and in his spare time studied for a London M.A. in Sociology and Ethics. He passed this, in 1912, with "Special Distinction". Initially Bartlett had, "no fixed intention to join University". But soon, on the basis of this academic success, he applied to become an undergraduate at St. John's, "largely because Rivers was there", (1936).

Thus Bartlett started on his Cambridge career in the Autumn of 1912. He had achieved his entrance to St. John's almost entirely on the strength of the efforts of his family, some family friends and himself. His formal instruction had been virtually non-existent and yet in ten more years he was to be leading the country's foremost psychological laboratory.

His story is all the more remarkable when one remembers that entry to university, let alone to Cambridge, was far more restricted than it is today. Bartlett had come from a relatively underprivileged rural background. He had had none of the private tuition and schooling then the norm for Oxbridge entrants. Later Bartlett was never exactly enamoured with the social and intellectual constraints of Cambridge. This, as Oldfield (1972) concurs, was probably in no small way, due to his upbringing

in the Cotswolds which were then well known as a centre of liberalism and religious non-conformity. Bartlett carried from his childhood, as Oldfield writes (op.cit), "a happy endowment of freedom from the social and intellectual constraints of the London-Oxford-Cambridge triangle". Bartlett's rather rustic speech, which he always retained, and his rather eccentric management of this department both tend to support Oldfield's claim. But certainly Bartlett was always most proud of his Cotswold origins.

One last point of interest derives from Bartlett's childhood, namely his early association with Liberal politics and non-conformist religion. This was important to Bartlett for, as he put it himself (1955(1)),"At the beginning of the century psychology ... was usually regarded as .... either the sport of rather cranky people or as a serious threat to the dignity of man". In other words the early psychologists had to be people with fairly firm, and rather unusual, intellectual opinions. They also had to be entirely unconvinced by the pervading religious reaction against psychology which we have encountered earlier in this work. On top of this they had to be adventurous enough to base their career within an embryonic, controversial discipline whose future then seemed far from certain. Bartlett <u>was</u> such a person. We cannot discount his father's role in this for he was an "outspoken and deeply convinced Liberal".

Indeed all four of the main members of the Cambridge School, perhaps inevitably, shared views of a generally liberal and progressive nature - at least in the first part of their careers. Rivers and Ward, as we have seen, were firmly committed to liberal politics, while Ward, Myers and Bartlett all emerged from strongly non-conformist backgrounds.

B. At Cambridge 1912 - 1914

On the basis of his previous work Bartlett was allowed to complete the Moral Science Tripos in just two years. He passed with a First in 1914.

Bartlett's "Director of Moral Science Studies" at St. John's was W. H. R. Rivers. "Already," writes Bartlett, "mainly through Dr. E. O. Lewis, who was a good friend to me, I knew something of the work of Dr. W. H. R. Rivers and I admired it greatly". Rivers soon drew Bartlett firmly towards anthropology and psychology - despite the fact that Rivers seemed to spend "all his time either writing or travelling to the South Pacific" and consequently did not see Bartlett all that much!

Bartlett's introductory lectures in psychology "were all highly theoretical" and given by MacTaggart, Johnson, Sorley, Ward and Dawes Hicks. Early on in his undergraduate career he met Moore and Russell, then at their most devastating, at a "Squash" held by MacTaggart.

> "There was a prolonged discussion on whether the rats said to be seen in an advanced state of delirium tremens were real or not. MacTaggart thought they were not real; Moore argued that they were not real in any ordinary sense of the word and Russell rather played one off against the other. When I was appealed to as a sort of umpire I know

I was completely flummoxed and I believe
I tried to agree with them all. Anyway
I came away feeling very depressed".

Soon after this Bartlett defended the, then very unpopular, thinker Bergson at the Moral Sciences Club. These two incidents seem to have ended any inclination Bartlett might have had towards philosophy. But soon he was promoted into Dawes Hicks' class on "Advanced Psychology". For most of this time," writes Bartlett, "I was the only student at this course. It was given in a large lecture room and Dawes Hicks spoke in a loud voice with a far distant look in his eyes and at dictation speed".

Much of this introduction to psychology was, writes Bartlett, not psychology at all but epistemology and concerned itself with the nature of sense data and language. In May 1913, Bartlett was awarded a Foundation Scholarship from St. John's. In the following Autumn he became more involved with psychology, taking experimental classes run by Myers and his assistant Cyril Burt. He was immediately attracted, finding that the course was well and informally led by Burt. "It was Germans, Germans all the way," wrote Bartlett (1951(4))"and if we were going to stick to psychology then to Germany sooner or later we must all surely go". He covered Helmholtz, Hering, Wundt, Blix, Goldscheider, Van Frez, Müller, Kraepelin, some "refreshing excursions" into the Wurzburg work and some psychometric tests devised by Burt. However, Bartlett writes, "in general I found the experimental approach vastly

attractive and I came to the end of this year feeling sure that psychological experimentalism was very much more in my line than philosophical speculation". It should be remembered, of course, that although much of Bartlett's early training derived from German work it was heavily coloured with Myers' own interpretation. One only has to read the preface to his Introduction to Experimental Psychology to realise how far Myers had travelled from the original German studies. Bartlett received no mindless trotting out of Wundtian introspectionism.

In this, his last year of training, Bartlett had further contact with James Ward. "Professor James Ward," he writes,

> "had a good class of about fifteen people and I think we all loved it and him, I did anyway....Ward was already in the later seventies when I came to know him, and he had moved away from psychology towards an idealistic type of philosophy which was fast losing any wide acceptance. Nevertheless when, as was by no means difficult, he could still be induced to talk about psychology, he was magnificent compared with any of his predecessors in Britain. He never failed to stress the importance, for an understanding of any form of behaviour, of a fundamental activity. This, he maintained, could not be properly analysed into specific items of experience, and it was this, mainly, which gave to behaviour its directional qualities..... Occasionally he would come into the laboratory, but he was unhappy there and never stayed long. He regarded all the experments we did as "psycho-physics" rather than as proper psychology..... but all the same he exercised a profound influence in two closely related ways; first by insisting that the experimentalist in psychology cannot afford to live only in a laboratory, and secondly that he must maintain respect for the integrity of human activity".

Bartlett took his final exams in the Moral Science Tripos in the Summer term of 1914. He was placed in the First Class. But, even before the results were announced, he was approached by Rivers. He told him that the post of Assistant in Experimental Psychology was shortly to fall vacant. The incumbent, Cyril Burt, was leaving and Rivers indicated that Myers wanted Bartlett to replace him. Bartlett was very unsure about the proposition. He was, "torn between psychology and anthropology" (Bartlett 1936). But he was persuaded, by Rivers, that a further training in experimental psychology would be very useful even if he eventually ended up in anthropology. When he did decide to take the post, he continues (1936), "it was in my plan to go in for field work in anthropology after a few years more intensive study of method in the laboratory". Later, in 1969, he reflected, of this move; "So I was to become a professional experimental psychologist, having attended one elementary practical course and listened to a few lectures on psychology, most of them highly theoretical". As we now know he remained a "professional experimental psychologist" for the rest of his days. But as we shall see below the "vivid personality" (Bartlett 1923(1)) of Rivers left its mark on Bartlett. For, rather surprisingly, in retrospect, anthropology and cross cultural psychology occupied a full half of Bartlett's publications for the next twenty years - an aspect of his career that is seldom appreciated.

C. The War and After 1914 - 1922

Bartlett was just settling into his new, £175 per annum, post when War broke out. His boyhood attack of pleurisy rendered him ineligible for military service. But very soon Myers was off to France to deal with shell shock. Shortly after, Rivers left to help out at Maghull and even Bernard Muscio, the Demonstrator, departed during 1916, for his native Australia.

As we have seen, Cyril Burt had also just vacated his Cambridge post, leaving Bartlett without a senior in Cambridge for practically the whole of the War. In recognition of this state of affairs he was created Temporary Director. Thus Bartlett began his first spell at the helm of Cambridge psychology - through an accident of illness "one elementary practical class" and "a few lectures" in a subject which he regarded, in any case, as second best to anthropology.

As we have seen in Chapter 9 the laboratory operated at a low level throughout the War owing to the obvious lack of staff and students. Nevertheless the period provided valuable experience for Bartlett, both in the administrative field and in academic matters.

Administratively Bartlett proved himself competent in handling the organisation of the courses and research of the laboratory. Certainly Myers went to some lengths, in 1922, to ensure that Bartlett succeeded him - suggesting that he was more than pleased with his performance.

Academically Bartlett's work progressed on four fronts:

1. From 1914-1916 he worked on his fellowship dissertation "Transformations Arising from Repeated Representation; a contribution towards an experimental study of the process of conventionalisation". This study was the work from which Remembering eventually developed. The work is particularly interesting for it shows the anthropological roots of much of Bartlett's thinking. It also shows the influence of Ward in its stress on an individual's active participation in how he symbolises the external world. But this work is discussed later, in its proper place, Chapter 15.

The upshot was also significant for Bartlett himself, as he acknowledged;

> "In 1917 I was elected to a Fellowship at St. John's College and I have continued to hold this ever since. The election was a very great relief to me, for at the time I was rather depressed about my probable future. But now I could reckon on continuing for some time in Cambridge".

2. From 1917, Bartlett was also involved in the hydrophone work which is described in Chapter 9. This work not only introduced Bartlett to applied psychology, but also to a Miss E. M. Smith. Before the hydrophone unit was disbanded, wrote Bartlett;

> "Miss E. M. Smith, who had been working on Animal Behaviour problems in the Cambridge Psychology Department for a number of years, and was a Fellow of Newnham College, joined me in experiments

> on listening to sounds of very weak intensity. Our results were published later, but the more important upshot was that we became engaged and were married in 1920, to the lasting satisfaction of us both".

3. Throughout the War Bartlett also assisted in the treatment of shell shocked soldiers at the Eastern General Hospital, near Cambridge (see Chapter 9).

4. Lastly, Bartlett pursued his early interest in philosophy - an interest which he dropped immediately after the War. The years 1913 and 1914 had seen the publication of Exercises in Logic and the Key to Exercises in Logic. Rather strangely these were Bartlett's first publications. Both were useful, small, introductory text books. The former proved so useful it was reprinted in 1926, 1948 and 1955. In 1917 and 1918 Bartlett presented papers on "Valuation and Existence" and "The Development of Criticism" at the Aristotelian Society. But these papers marked the end of his commit ment to philosophy per se. After this his interest in philosophy usually only concerned questions of scientific method in psychology and, to a lesser extent, in the other social sciences . (e.g. Bartlett 1927(2), 1929(2), 1937(2), 1939, 1945(3), 1951(1), 1955(1)).

Rivers and Myers both returned to Cambridge in 1918. The impact of the War on these two men and the progress of Cambridge psychology until 1922 has been recorded above. Bartlett himself was relieved of most of his administrative burden and once more came under the influence

of Rivers. During the immediate post-war years he continued to pursue the developments of the hydrophone research (1919, 1922 (1)) and his fellowship dissertation (1921). But a large chunk of his effort was now devoted to his reawakened interest in anthropological studies. He published papers on folk stories (1920 2 &3) and in 1922 delivered a series of anthropologically inclined lectures at Bedford College, London. The following year these lectures were published as Psychology and Primitive Culture - Bartlett's first major book on psychology. Zangwill and Harris (1973) say of the book that it was "in some sense a reflection of Bartlett's own unrealised anthropological vocation...(and)... an early contribution towards....cross-cultural psychology".

But we go too fast. For, in 1922, as we have seen (Part 3) Rivers had died and Myers had left Cambridge. Bartlett, at the age of 36, was again left without a single senior in psychology at Cambridge - with the exception of the now thoroughly *un*interested James Ward. Thus Bartlett, with the considerable assistance of Myers, was elevated to the posts of Reader in Experimental Psychology and Director of the Psychological Laboratory with their attendant stipend of £650 per annum. He remained Cambridge's senior psychologist for the next thirty years.

D. Bartlett as Head of Cambridge Psychology 1922-1939

Above, we have related the rather unusual circumstances which brought Bartlett to the helm of Cambridge psychology. He found himself, as a fairly inexperienced thirty six year old, in a position vital to British psychology's future. For as we shall see below (Chapters 13 and 16) Cambridge was to become something of a fountainhead for the wisdom of British psychology and the major source of its professors.

In fact most of Bartlett's activity in this, probably the busiest, section of his life is dealt with in the following chapters. He played four major roles during this period. Firstly he was an experimental psychologist (Chapters 14 and 15), secondly he was an unusually successful and influential teacher (Chapter 13), thirdly he guided the growing Cambridge Laboratory (Chapter 13) and last of all he served on several influential government and N.I.I.P. committees concerned with the growth of applied psychology (see Chapter 13).

In brief, Bartlett made a major impact in all four of these roles. He produced, in Remembering, the most notable, and certainly the most lasting, contribution to British psychology between the Wars. As a teacher he produced, as Broadbent puts it (197C(2)),"a brilliant crop of students who, after the Second World War, held the lion's share of the Professorships of Psychology in Britain; and indeed quite a number elsewhere in the

Commonwealth". The laboratory grew and prospered under his leadership. In recognition of this Bartlett himself was elevated to Professor in 1931 while his laboratory gained four new teaching posts before the War.

In 1924 Bartlett became Editor of the British Journal of Psychology. He held this post until 1948 - a length of tenure unlikely to be repeated! In 1929 he was elected President of the Psychology Section of the British Association - his address on this occasion being entitled "Experimental method in psychology". In 1932 Bartlett joined Spearman, McDougall and Myers in the ranks of the Fellows of the Royal Society.

By the time War broke out Bartlett was firmly established as one of the country's leading psychological authorities. Moreover he had created a reputation for himself as a more than able committee man. Thus it was hardly surprising to find Bartlett himself serving on several important War time Government committees and much military research work being conducted at his laboratory.

However a consideration of Bartlett's War work - important and interesting though it is - is far beyond the scope of this thesis. Bartlett's life and work from 1939 onwards is however covered in brief in our Postscript (see Part 5).

CHAPTER 13

THE CAMBRIDGE LABORATORY 1922-1939; BARTLETT THE TEACHER AND ADMINISTRATOR

A. Psychology at Cambridge in 1922

As we have seen in Part 3 Bartlett began this period as Reader in Experimental Psychology. MacCurdy was Lecturer in Psychopathology and Sprott was the Demonstrator. In fact 1922 marked a considerable shake up for the laboratory. Myers and Rivers had departed, Ward was becoming increasingly disabled and disinterested. Muscio, Prideaux and Thouless, who had all contributed to research and lecturing, went their various ways.

Cambridge psychology had, since 1920, been under the control of its own Board of Studies. In the Autumn of 1921 the Board offered its first lecture programme under the chairmanship of Myers (see photo on following page). The increasingly old fashioned philosophical approach was still represented with courses from G. E. Moore and Dawes Hicks. But in general the experimental, physiolgical, industrial and clinical approaches were now rather more in evidence:

> "Myers had arranged that the Psychological Department, though entirely independent as regards its direction and policies should be actually housed in the larger Physiology Laboratory. When Sir Joseph Barcroft took over as Head of Cambridge physiology the relations of the two Departments became very close",

thus wrote Bartlett (1969). This co-operation was later aided by the fact that Adrian and Bartlett were "very thick" (Rodger 1977).

PLATE 12

This photograph shows the first programme of lectures organised by the newly independent Board of Psychological Studies for the session 1920-1921.

Lectures proposed by the Board of Psychological Studies, 1920–1921

Except where otherwise stated the courses of lectures for each term will begin on **Monday, October 11**, **Monday, January 17**, *and* **Monday, April 25**, *respectively, or as soon thereafter as the days fixed for the lectures permit.*

Except where otherwise indicated all courses will be conducted at the **Psychological Laboratory.**

Michaelmas Term, 1920	Lent Term, 1921	Easter Term, 1921
Dr Myers. Principles of Mental Development. M. W. 5.30. Oct. 13.	Same continued. M. W. 5.30.	
Dr Myers and Mr Bartlett. Advanced Psychology with Practical Work. Th. S. 10; M. F. 2.30. £2. 2*s*.	Advanced Psychology with Practical Work (continued). Th. S. 10; M. F. 2.30. £2. 2*s*.	
Dr Moore. *Arts School.* Psychology. M. W. F. 11. £2. 2*s*. (p.)	Psychology (continued). M. W. F. 11. £2. 2*s*. (p.)	Psychology (continued). M. W. F. 11. £2. 2*s*. (p.)
Mr Rivers.		The Psychology of Dreams. S. 12. 10*s*. 6*d*.
Mr Bullough.	Theory of Art in Relation to Architecture[1].	Theory of Art in Relation to Architecture (continued)[1].
Mr Dawes Hicks. Advanced Psychology[2]. W. 3. £1. 1*s*.	Advanced Psychology (continued). W. 3. £1. 1*s*.	Advanced Psychology (continued). W. 3. £1. 1*s*.
Mr Bartlett and Mr Dawes Hicks. Elementary Psychology. M. W. F. 12. £2. 2*s*.	Elementary Psychology (continued). M. W. F. 12. £2. 2*s*.	Elementary Psychology (continued). M. W. F. 12. £2. 2*s*.
Mr Bartlett and Mr Lowson. Practical Work in Elementary Psychology. Tu. Th. 11—1. £2. 12*s*. 6*d*.	Practical Work in Elementary Psychology (continued). Tu. Th. 11—1. £2. 12*s*. 6*d*.	
Mr Lowson.	Social Psychology. Th. 5—6. 10*s*. 6*d*.	
Dr Anderson. *Physiological Lab.* The Nervous System. Internal Secretion. Tu. Th. S. 9. With practical work. M. F. 10. £3. 3*s*. to Physiological Dept.		
Dr Hartridge. *Phys. Lab.* Physiology of the Eye. W. F. 9. £2. 2*s*.	Physiology of the Ear, Nose, etc. W. F. 9. Demonstration, F. 2—4.	Physiology of the Sense Organs (elementary).
Dr Adrian.	*Phys. Lab.* Conduction in the Central Nervous System. M. 9. Practical Work. M. 12. £2. 2*s*.	Localisation in the Central Nervous System. M. F. 9. Practical Work. F. 2.30. £2. 2*s*.
Mr Lowson. The Central Nervous System and the Sense Organs[3]. M. S. 9. £1. 1*s*.		
Mr Fox. The Psychology of Education. M. 4.30, F. 5.30. £1. 1*s*.	Practical Work in the Psychology of Education[4]. £2. 12*s*. 6*d*.	
Mr Yule. Elements of Statistical Method. 10*s*. 6*d*. S. 11—12.	Elements of Statistical Method (continued). 10*s*. 6*d*. S. 11—12.	
Mr Muscio.	Psychology in Relation to Industry. Th. 12. Practical Work. M. 2—4. £1. 1*s*.	

[1] Once a week: times to be arranged.
[2] Students intending to take this course are requested to communicate with Mr Dawes Hicks, at 9 Cranmer Road, Cambridge, not later than Oct. 12, 1920.
[3] These lectures are intended for candidates taking the Special Examination in Psychology.
[4] Twice a week: times to be arranged.

Hamilton Hartridge was still lecturing in the Physiology of the Senses - on the eyes, ears, the nose and "The Sense Organs". Adrian gave courses on the central nervous system. Meanwhile MacCurdy began courses on "Dreams and the Unconscious". The applied field was covered; by a Mr. Cloake with lectures on "Social and Abnormal Psychology" and "Psychology in Relation to Health and Morale"; by Fox who offered "The Psychology of Education"- and by Fildes who continued her courses on "Mental Tests". Around 40 pupils now took psychology every year. Such, briefly, was the state of Cambridge psychology at the beginning of Bartlett's reign.

Bartlett saw the options open to him like this; op.cit.

> "I now had to make a definite decision about how I would try to develop the Laboratory. Two possibilities seemed to be wide open. I could set to work to build a large teaching department. There was, for example, a rapidly expanding Medical interest in psychology, due in part to the popularity of the work of Freud, Jung and some of their associates, but perhaps still more to what had been done by Myers, Rivers, Sir Henry Head and others. The Government established Medical Grants Committee was outstandingly friendly and willing to help. Or I could decide to keep the Department rather small, highly selected, and principally directed towards the promotion of original research".

Bartlett chose the latter course and it was one which, he wrote, he "never regretted". Indeed, as we shall see below, the number of students passing through the laboratory grew relatively little in these seventeen years - compared to the amount of research, contacts with grant providing bodies and the number of lectureships.

B. Psychology Becomes Accepted as an Applied and Natural Science

One of the first battles Bartlett took up was that to establish Psychology as a suitable subject for Part II of the Natural Science Tripos. As Bartlett himself put it (1969);

> "At this time an honours degree in Psychology in Cambridge was a Part II tripos subject only; that is to say a candidate must have taken already a Part I honours examination successfully in some other subject. Also it still remained a section of the Moral Sciences Tripos only and had a fairly strong philosophical bias. It seemed to me that we should remain as a Part II subject, but that it should become possible to take this as a section of the Natural Science Tripos. After considerable discussion and campaigning this was officially agreed, and from then on most of the candidates who entered for an advanced psychology degree had successfully completed an earlier training in physiology, physics or some other of the Natural Sciences".

The "considerable discussion" Bartlett mentions lasted until 1934! In fact the concern over psychology's status had started in 1920. Characteristically Myers, through the Psychological Board of Studies, had, in November of that year, argued the need for psychology to be incorporated into courses on Economics, Teacher Training, Medicine and Politics. A year later he set up a committee to discuss "The place of Psychology in relation to the Natural Science Tripos". Furthermore Myers had proposed lectureships in Industrial, Abnormal and Educational Psychology. Not surprisingly the Psychological Board strongly supported these moves. Clearly this internal campaigning plus the increase in social acceptance of applied psychology (see Chapter 10) tended to make psychology's place among the

philosophical Moral Sciences look more and more incongruous.

As ever, Cambridge was slow to react to psychology's claims - despite, as we shall see below, the ever growing government funding of applied research in Bartlett's laboratory. But the internal reorganisation of 1926 did provide a step in the right direction.

Under this scheme all the Special Boards (for established subjects) and Boards of Studies were abolished and a new two tier structure of Faculties and Departments was created. Under this scheme (Reporter 1926-1927 p. 343) a Psychology Department was created to be under the control of both the Faculty of Moral Science and the Faculty of Biology "B" (the latter having responsibility for physiology). This occurred, needless to say, despite a recommendation from the Psychology Board that the new Department should come under Biology "B". (Minutes book 28/9/1924).

However, from the Autumn of 1927, both Moral Science and Biology "B" offered their own courses in psychology. The total content was similar to 1922 - with the obvious biases towards the philosophical or physiological as appropriate. At the same time Psychology at last became a principal examination subject - but still in the Moral Science Tripos.

By this time the incongruity of psychology in the M.S.T. had become glaringly obvious. By the early 1930's Grindley and Blackburn were studying learning curves in chickens and rats while Magdalen Vernon was studying eye

movements in reading for the Medical Research Council - in a Department supposedly producing candidates for a Tripos in philosophical studies! In May 1934 Cambridge eventually bowed to the inevitable and revised the Natural Science Tripos. Now, for Part II, candidates could take two papers in Experimental Psychology instead of two in Physiology. This move had not come before time. It had effectively cut off all undergraduates with a natural science background from psychology. For instance in 1922 M. D. Vernon had graduated via the N.S.T. and she had to work her way into psychology via research. Who knows how many able candidates had been lost?

It was not until May 1935 that the first Psychology exams were taken in the N.S.T. Part II - just fifteen years after the Psychological Board suggested it.

C. Research

(Most of the information below comes from Bartlett's contributions to the "Annual Reports of the General Board of Studies". See the Reporter for the various years).

By March 1921, under Myers' guidance the Cambridge programme in applied psychological research was well under way. The I.F.R.B. had funded Bernard Muscio's industrial studies while the M.R.C. and the "Board of Control for Health" had provided money for the work of Fildes on assessment techniques and Prideaux on psychoneurotic disorders. Under Bartlett this programme continued and expanded.

In 1923 Bartlett was appointed to three important I.F.R.B. bodies. These were the committees for "Legibility of Type," "Industrial Psychology" and "Accident Causation". In 1927 he was appointed a member of the I.F.R.B. committee for the "Psychology of Vision" and the following year to the (now renamed) I.H.R.B. committee for the "Psychology of Hearing". He writes (1969) that, "the Medical Research Council, and for several years its associated Industrial Health Fatigue Board[1] did far more to aid the growth of experimental psychology through Cambridge than any other outside agency".

Thus it is hardly surprising to find much Government funded research being conducted at Cambridge in these inter war years. (In fact the relevant papers have not been traced by the current author. But Bartlett repeatedly refers to "Government Committees" and "the M.R.C." in his annual reports and, bearing in mind his growing influence during these years, it seems likely that most of the work listed below was directly funded by the I.F.R.B./ I.H.R.B.) During the immediate pre-war years Edward Mellanby (later Sir) became Secretary to the M.R.C. and also a firm friend of Bartlett's. These two were in the centre of the wartime experimental research and the resultant setting up of the M.R.C. Applied Psychology

(1) Bartlett is of course referring to the Industrial Fatigue Research Board and, after 1928, the Industrial Health Research Board.

Unit at Cambridge (see Part 5). But the foundation for the famous Cambridge involvement in World War II was laid in the 1920's and 1930's.

In 1924 Bartlett reported (p. 728) that research on "legibility of type" was being conducted by R. L. Pyke and M. D. Vernon. Vernon in fact was to spend the majority of the next fifteen years involved in this research which was largely M.R.C. funded.(1) Later she took in the study of eye movements and even made one of the first British reviews of rapid reading techniques (1931). Other Government funded work included research on "general motor ability," "long spells of repetitive work on motor accuracy" and "rhythmic changes of mental and muscular efficiency". Meanwhile Harry Banister had just begun his long association with the laboratory. He was accepted, under the supervision of Hartridge, to study for a Ph.D. on binaural localisation. This he duly achieved (see Banister 1922, 1923 a and b). He was of course continuing the theme of Bartlett's own hydrophone work originally conducted for the Navy, (but the funding is uncertain).

Regarding more orthodox psychology various students dabbled in "animal psychology" but nothing of note was achieved in this field until the arrival of Gwilym Grindley in 1929. Grindley published several interesting papers,

(1) Vernon was in fact "Assistant investigator" for the I.H.R.B. from 1924 to 1927. From 1927 to 1946 she was "Research Investigator" for the M.R.C. at Cambridge.

mainly in the British Journal of Psychology, during the 1930's. In fact, as Hearnshaw has indicated, Grindley quickly became Britain's expert on animal conditioning studies. He began in 1929, with papers on "Pavlov's Conditioned Reflex" and "Experiments on the Influence of the Amount of Reward on Learning in Young Chickens". Grindley's work was arguably the first major British contribution to this field since E.M. Smith's 1915 Mind in Animals.(1)

In 1925 Bartlett reported that G.W. Allport had conducted research on eidetic imagery. Government work, conducted by Pyke and Mathews, on legibility continued while L.C. Baker pursued one of Bartlett's pet topics "mental imagery and motor skill". The following years followed much the same pattern. The first Cambridge research into vocational guidance, by L.C. Ramsay began in 1926. The academic year of 1926-1927 saw the publication (in the B.J.P.) of papers on "Perception of Almost Inaudible Sounds" by R.W. Pickford and "Transfer Learning in Guinea Pigs" by C.V.D. Hadley. The following year saw the first published work by P.E. Vernon on personality. Entitled, "Tests of Temperament and Personality," it appeared in the B.J.P. This work, together with that of O.A. Oeser really constituted the major breakaway group from what was now traditional Cambridge psychology.

It will have been noted from the above that firm patterns of research began to emerge in the 1920's. Work concentrated on:

a) Applied studies - industrial and clinical.

(1) Grindley reviewed his work in The Intelligence of Animals (1937)

b) Perceptual and cognitive studies.

c) Animal psychology - the characteristics of learning curves and reinforcement schedules.

Bartlett's reports for the 1930's are rather less detailed than for the 1920's. Also there appears to be no documents relating to departmental research available in the laboratory's archives. However the three main pieces of Cambridge work during the 1930's were; Bartlett's own which culminated with Remembering in 1932; M. D. Vernon's well known studies which expanded from reading and explored the whole field of perception usually showing pronounced Barlettian influence; lastly Grindley's work on animal learning. However the M.R.C. funded work continued apace. R. H. Thouless returned to the department to find that the M.R.C. "had rather taken over the place" (1977). This recollection concurs with that of Alice Heim, who was a Cambridge student from 1931 - 1934, before undertaking an M.R.C. funded Ph.D. under Eric Farmer (Heim 1977).

With the very notable exceptions of Bartlett and M. D. Vernon the Cambridge research of 1922 - 1939 was workmanlike rather than world shattering. This is not surprising as much of it was applied research dealing with ad hoc problems and therefore not likely to produce great theoretical advances.

D. Lectures and Staff

The Psychological Laboratory's staff expanded from three in 1922 to six in 1939. Banister joined the staff,

as Demonstrator, on 25th March 1925 replacing Sprott who left to lead the new department at Nottingham. The following year his post was upgraded to Lecturer in Experimental Psychology. Banister, MacCurdy and Bartlett assisted by the outside lecturers mentioned above saw out the 1920's.

The 1930's however brought about considerable change in the department. On 24th November, 1930 the General Board of Studies reported to the Council of the Senate that;

> "The board have had in mind for some considerable time the position of the Reader in Experimental Psychology. As head of a large laboratory, which was in 1926 created a department under the New Statutes, Mr. Bartlett has very considerable administrative responsibilities. Since the establishment of the Readership in 1921 this subject has steadily increased in scope and importance and there is at present in the laboratory of Experimental Psychology a flourishing research school which attracts students and research workers from many countries, a result attributable in no small degree to the distinction of the present Reader".

The Board went on to recommend the setting up of a Chair in Experimental Psychology. Both the boards concerned, those of Moral Science and Biology "B""warmly approved" the suggestion. Furthermore when the proposal passed the Council of Senate "no remarks were made" - a sure sign of the changing times.

Thus Bartlett became the first Professor of Experimental Psychology at Cambridge, from January 1st 1931. His

stipend was now £1,200 and as a further indication of psychology's new position his Chair was formally attached to Biology "B".

In 1930 only T. H. Pear, at Manchester, and Spearman, at U.C. London, held chairs in psychology in the U.K. By the end of 1931 Drever had been installed as Edinburgh's first Professor along with Aveling at King's College, London, and Burt had replaced Spearman.

On March 4th 1931 the General Board received a letter from the Board for Biology "B". Part of it read;

> "The classes in the Department of Experimental Psychology have increased so considerably both in diversity and in number during the last few years that it has been necessary each year for an amount of informal help more than enough to cover the normal basic work of a University Demonstrator".

This scarcely veiled appeal for the establishment of a new Demonstrator's post was agreed to by the General Board. On October 1st, 1931 G. C. Grindley who, as we have seen, had been researching in the Department, was appointed to the new post.

The "considerable increase" in numbers of students was very real. The average of 40 students per term in 1921 grew to 65 in 1925, to 75 in 1927 and to 90 in 1931. Yet the number of strictly psychological staff was still the same! This level of activity continued throughout the 1930's but the diversity of courses, as we shall see below, continued to develop. There was a clear need for extra staff.

Thus Eric Farmer, who had graduated through the Moral Science Tripos in 1913, and had worked with Myers at the N.I.I.P. since 1924,[2] was recalled to Cambridge. He was appointed to a new Readership in Industrial Psychology on 15th June 1935. This post was funded by the M.R.C. Farmer will be best remembered for his work with Eric Chambers on the causation of industrial accidents. Chambers in fact joined the Cambridge Department, on the very same day as Farmer, as Assistant Director of Research in Industrial Psychology. Also in 1935 Grindley was promoted to become a Lecturer in Experimental Psychology.

The last addition to the staff before the War was R. H. Thouless. He was created a Lecturer in Educational Psychology in 1938. Thouless had graduated at Cambridge in 1912 and after a spell in Manchester had recently been leading the Psychology Department at Glasgow - after the death of H. J. Watt.[1]

(1) Thouless (along with Oeser and P. E. Vernon) was also never really accepted by Bartlett. Thouless had written a Ph.D. on the psychology of religion (examined by Sorley and Ward), was a pioneer of psychological statistics and furthermore was interested in parapsychology. These topics were far from being Barlett's favourites and relations between the two were never good.

Although he contributed considerably to the Laboratory's lectures Thouless was not officially attached to the Psychology Department.

(2) Although employed for long periods by the I.H.R.B. Farmer and Chambers both undertook work on contract to the N.I.I.P.

Not surprisingly these seventeen years saw a considerable change in the nature of the lectures given in the Psychology Department. Throughout this period there was an increasing diversity of topics and a pronounced shift towards the experimental and applied from the metaphysical. As early as 1923 Bartlett began to lecture on Health and Morale, Social Psychology and Industrial Psychology and Measures of Efficiency. In the same year MacCurdy expanded his courses to include "Psychopathology" and "The Mind of Primitive Man". The following year Sprott began to teach "Theories of Perception With Special Reference to Experimental Work" and Bartlett weighed in with 'Psychology and War".

In fact the last of the old systematic psychology had very nearly disappeared by 1927. By then the new syllabus for psychology as a Principal examination subject was;

1. Physiology of the C.N.S. and sense organs - including the reflex arc.
2. General Psychology - including reflex activity and combinations, imagery and imaginative thinking.
3. Abnormal OR Industrial Psychology.
4. Practical work.
5. Essay question.

In fact Wardian psychology was taught well into the 1930's - usually through Stout's still widely used book.

But by now it only formed a small part of the syllabus. In 1930-1931 the courses were dominated by Banister's and Bartlett's lectures on experimental work in psychology. MacCurdy continued his clinical teaching and Fox his educational lectures. Adrian still contributed his work on conduction in the C.N.S. Dawes Hicks was by now the only remaining representative of philosophical psychology lecturing on "Personality and Volition".

Big changes however followed the arrival of Farmer and Chambers in 1935. Now in addition to the previous programme Farmer lectured on "Industrial Psychology" and Chambers on "Applied Statistics". M. D. Vernon began lecturing on the "Experimental Psychology of Cognitive Process". Just before the War two new young research students began to give lectures. One, R. C. Oldfield began teaching "The Experimental Psychology of Thinking" while the other, O. L. Zangwill, began with "The Experimental Psychology of Perceiving".

E. Bartlett the Teacher - Institutional Factors

Tables 4, 5 and 6, on the following pages attempt to indicate the quite extraordinary place which the Cambridge laboratory holds in British psychology. Broadbent (1970(2))hinted at Bartlett's role in this phenomenon;

> "Frederic Charles Bartlett was the first Professor of Experimental Psychology in the University of Cambridge. As such he exerted a crucial influence over the development of the subject throughout the whole country: his students staffed the newer departments which have since arisen, and his approach has coloured all subsequent research in England. His role in the subject was therefore unique".

In fact under Bartlett's guidance the role of the Cambridge laboratory was one which can only occur once in the growth of a new discipline. Before the Second World War there were but six chairs in psychology and an estimated thirty lecturers in the whole of the U.K. British psychology was indeed a small world.

But the discipline began to expand rather more swiftly after the War. By 1947 there were ten chairs and by 1957 there were sixteen. If we look at the postgraduate training of the sixteen Professors of Psychology in 1957, ten had been trained by Bartlett and Myers and three by Cyril Burt or Spearman. Russell had arrived from America. While of the remaining two Smith had arrived at Birkbeck via his native Australia and Hearnshaw had been trained at King's College by Aveling.

The effect of this expansion had been to make universities look around for experienced psychologists - ones who had been trained say, some twenty years ago, in the 1930's. At <u>that</u> time Cambridge and University College, London were the biggest, best equipped and most prestigious laboratories. Only Manchester and latterly Edinburgh were really in any position to compete. Thus, in the 1930's, Cambridge and U.C.L. had tended not only to produce the majority of bright young psychologists but also had been able to attract the brightest postgraduate workers from elsewhere.

The effect of this, as Table 6 shows diagramatically, was to produce a "Bartlett cluster" and a "Burt cluster" of pupils. These pupils were the best equipped candidates for

TABLE 4

SOME CAMBRIDGE TRAINEES IN PSYCHOLOGY 1912-1950

1912 R. H. Thouless N.S.T. Arnold Gerstenberg Scholar.

1913 Eric Farmer M.S.T.

1914 F. C. Bartlett M.S.T. (Professor at Cambridge 1931-1952).

1915 C. A. Mace (Professor at Birbeck 1944-1961).

1922 W. J. H. Sprott M.S.T. (Professor at Nottingham 1960-1964).

Eric Chambers M.S.T. (Lecturer at Cambridge).

M. D. Vernon N.S.T. M.A. (Professor at Reading 1956-1957).

1927 P. E. Vernon M.S.T. (Professor at Institute of Education 1949-1968).

R. W. Pickford M.S.T. (Professor at Glasgow 1955-1973).

1928 Alec Rodger M.S.T. (Professor at Birbeck 1960-

1933 Alice Heim M.S.T. (Lecturer at Cambridge).

1934 James Drever Junior M.S.T. (Professor at Edinburgh 1944-1966).

1935 O. L. Zangwill M.S.T. (Professor at Cambridge 1952-

1937 R. C. Oldfield Research Student (Professor at Oxford 1956-1966).

A. T. Welford M.S.T. (Director of Nuffield Ageing Unit).

1940 K.J.W.Craik Ph.D. 1940 (Director designate Cambridge A.P.U.).

1948 R. Conrad M.S.T.

1948 D. E. Broadbent M.S.T. (Director Cambridge A.P.U. 1958-1974).

1950 Michael Argyle M.S.T. (Reader at Oxford)

R. Gregory M.S.T. (Professor of Neuropsychology Bristol 1970-)

A. T. Singleton M.S.T. (Professor at Aston 1965-)

M.S.T. - Moral Science Tripos.

N.S.T. - Natural Science Tripos.

Norman Mackworth, Julian Blackburn, Dan Berlyne, Rex Knight, D. W. Harding and George Drew, all later Professors, passed through the laboratory during this period, in addition to those above.

TABLE 5

ACADEMIC BACKGROUND OF UNIVERSITY PROFESSORS

OF PSYCHOLOGY IN THE U.K. 1937 - 1977

(a) BY POST GRADUATE TRAINING

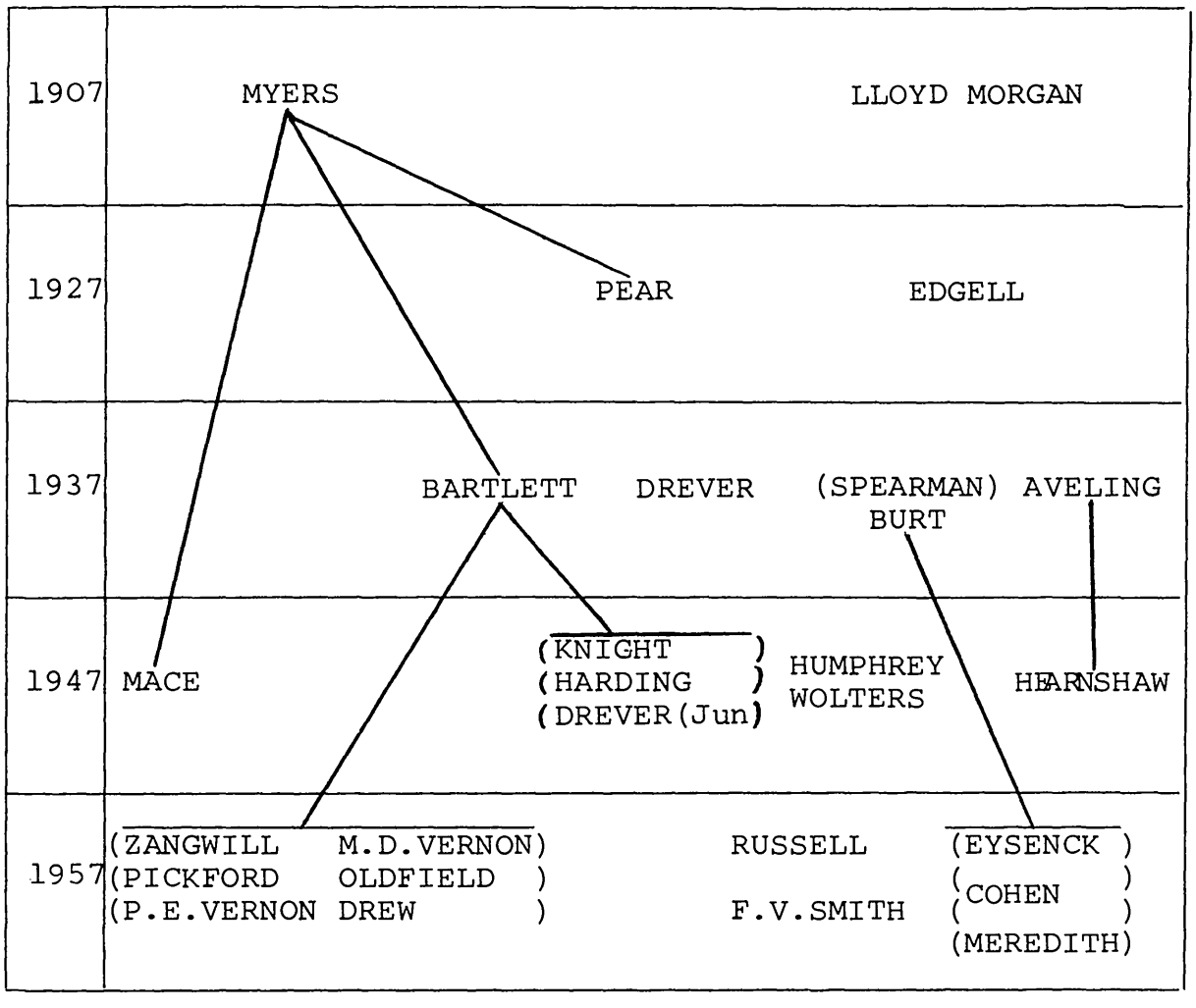
(b) BY UNDERGRADUATE DEGREE (in brackets)

	CAMBRIDGE	OXFORD	LONDON	OTHERS	TOTAL
1937	1 (1)	1 (1)	1½ (2)	1½ (1)	5
1947	5 (3)	1 (3)	1 (1)	3 (3)	10
1957	10 (7)	0 (1)	5 (2)	1 (6)	16
1967	9 (9)	8 (6)	16½(12)	3½(10)	41 2 Vacant 2 Unknown
1977	13(10)	10(10)	19½(16)	14½(21)	59 2 Unknown

TABLE 6

"FAMILY TREE" OF BRITISH PROFESSORS OF PSYCHOLOGY - BY POSTGRADUATE TRAINING

1907 - 1957



NOTES 1) This chart includes all U.K. Professors of Psychology prior to 1957.

2) Spearman was, of course, Burt's immediate predecessor at U.C.L.

the chairs of psychology which were instituted during the 1940's and 1950's. Thus perhaps it is not surprising to see that Bartlett's and Burt's pupils tended to monopolise British chairs until the 1960's when this stranglehold began to relax.

The 1960's were a time of great expansion in British psychology. As Hearnshaw (1969) has pointed out, membership of the B.P.S. increased threefold as did the yearly output of graduates. No less than twenty nine new chairs in psychology were created between 1957 and 1967. By now of course Universities had rather more choice in the background of their candidates and the proportion of new professors from Cambridge and U.C.L. substantially declined. However, interestingly enough, the two new main providers of professors in the 1960's were Oxford and the other London colleges. At Oxford psychology was led by R. C. Oldfield while the London colleges were led by Mace(Birkbeck), Harding (Bedford) and P. E. Vernon (Institute of Education)- these men were <u>all</u> trained by Bartlett, except Mace who was a pupil of Myers! This phenomenon has been alleged by John Annett (1977) to have produced a crop of "second generation Bartlettians".

It is also salutary to note that these London Colleges together with Oxford and Cambridge produced over two thirds of our current fifty nine professors. Even more surprising is the fact that no less than twelve of these received significant training from Bartlett while seven did so from Cyril Burt.

There are of course drawbacks to the above analysis. Not the least of these is the fact that being taught by someone by no means implies allegiance. For example P. E. Vernon owes far more intellectually to the Spearman/ Burt tradition than to that of Bartlett. Nevertheless it is possible to see, from the above, how four of the major pioneers of British psychology, Myers, Bartlett, Spearman and Burt set up teaching institutions and then produced a flock of pupils who, until the late 1960's, monopolised British professorships.

I am at pains to point out that this does not necessarily mean that these pupils blindly followed their masters' every edict. But equally it would seem just as ludicrous to suggest that this training and placing of pupils did nothing whatever to promulgate the ideas of Bartlett and Burt.

F. Bartlett the Teacher - Personal Factors

We have seen in the previous section how various institutional factors conspired to place Bartlett's pupils in influential positions in British psychology. But these factors together with the prestigious nature of Cambridge University do not tell us the whole story of Bartlett's almost uncanny success as a teacher. There is clearly something missing.

Let us look at the comparable case of Cyril Burt. University College in the 1930's had, just like Cambridge, a fairly large, well equipped laboratory. It was also,

just like Cambridge, an old, well established, prestigious University with plenty of kudos with which to attract students and place them afterwards.

Yet Burt, although certainly Bartlett's equal intellectually, and a psychologist whose contribution per se was just as important and lasting, has no such remarkable teaching record.[1] Part of this discrepancy is probably due to Bartlett's considerable concern with "empire building". It is perfectly true that Bartlett was extremely concerned to place his pupils in influential positions. This was especially so for those who, by and large, adhered to his conception of psychology. Those who dabbled in the areas in which Bartlett was not so keen (that is in physiological, social, psychometrics and animal studies) were usually less enthusiastically sponsored. Bartlett was also extremely effective at behind the scenes manoeuvres whether it be for Government committees or for placing old pupils and colleagues (Rodger 1977).

Burt shared little of this interest and, it is probably fair to say, little of Bartlett's gift for pulling strings.

However the majority of Bartlett's success as a teacher is probably best attributed to his personality and teaching style. Let us examine some of the accounts left to us by his grateful ex-pupils. Donald Broadbent, arguably Britain's foremost living experimental psychologist writes;

> "Bartlett's writings would frankly not explain his extraordinary dominance inside his profession. It was rather his handling of everyday

[1] It has been pointed out to me that this analysis is less than fair to Burt. His pupils followed careers in more varied subjects than did Bartlett's. Also rather more of them seem to have gone abroad. Hence they tend not to appear in my figures. (I am indebted to Professor Hearnshaw for this information.)

relations in his department which gave him influence. He taught informally, thowing out ideas faster than they could be appreciated, encouraging students to think for themselves. His whole method supposed that they would read up factual information, that they would come to him with ideas of their own, that the details of human behaviour in everyday life would provide a constant fund of illustration and stimulus. It was incredibly easy to approach him, both because the modern hurdles of secretaries and anterooms were missing, and also because of the beaming and attentive welcome with which he would greet brash and ignorant potential students or importunate and worried postgraduates. His weekly lecture-discussions were a festive performance which nobody would have dreamed of missing, so that every corner of the small room was crammed. Out of hours, the staff and students might meet for tennis at his house: mixed with discussion with the Professor and Lady Bartlett, herself one of the earliest members of the laboratory. On another occasion as many as possible might go to Louvain to pay a visit to his old friend Professor Michotte. Some of the stories about his handling of departmental paper-work are, one hopes, apocryphal; but there is no doubt that to him people were always more important than forms, timetables, or academic syllabuses".

Tom Singleton, Professor at Aston since 1965, adds;

"I first met Sir Frederic Bartlett on a damp cold Cambridge evening in the Autumn term of 1948. He was to talk to the Moral Sciences Club in Trinity College. He came into the room without an overcoat but with a thick Johns scarf wrapped round his neck, hands in the pockets of his sports jacket but thumbs outside, all three buttons of the jacket fastened. He was tall with a powerful frame and rather sloping shoulders, a bald head with wispy grey side hair, a big nose and deep-set eyes under very bushy brows. At that time I was reading physics and I don't think I had ever heard of him before but I still have a clear visual image of the way he came into that room, unwrapped the scarf and laid it along the table.

> He talked, as far as I remember without notes, about democracy and delegation, about the difficulties of delegates who come together, appreciate each others' problems, reach agreement and then have to go back and justify what they have agreed to their parent bodies. He saw the problem in anthropological terms.
>
> Some time later I went to see him about switching to psychology and went through one of those famous interviews so well described by Conrad (Sir Frederic Bartlett - A Personal Homage, Ergonomics, January 1970). This resulted in my attending the long vacation course in psychology the following summer which was conducted by Bartlett himself. By current standards it was an extraordinarily unprofessional sort of teaching. For about six weeks we spent the mornings discussing psychology with Bartlett, the afternoons either on the river or playing tennis and the evenings in reading and talk. The modern educational accountant would get some dismal answers from his cost-effectiveness equations for this situation, yet of less than a dozen students I can think of about five who are now professors of psychology". (from Buzzard 1971).

In fact Buzzard's (1971) obituary is nothing less than a collection of tributes to Bartlett's teaching style. Dr. Alice Heim contributed the following to the collection;

> "Professor Sir Frederic Bartlett embodied the reconciliation of opposites. He was endlessly kind, yet he 'saw through' even the most opaque of people- he was a good politician and yet he never lost his intellectual integrity; he was enthusiastic and encouraging, yet shrewd and realistic in his assessments; his own approach to psychology tended to be holistic yet, as a critic, he was almost obsessively perspicacious, managing always to see the trees as well as the wood; his teaching mainly concerned the larger issues in human psychology, yet he inculcated ineradicably in his students the principles of experimental methodology. The truth of the last point is reflected in the fact that all the founder-members of the Experimental Psychology Society ('Group' when founded) were ex-pupils of his, and the majority of those now holding chairs in departments of experimental psychology were trained either by Sir Frederic or by one of his ex-students.

(See over)

Considering his degree of power and his outspokenness, Sir Frederic was remarkably well-liked by all the members of his laboratory - from workshop technician to university reader. This was due, probably, to his tolerance and his humour. He was keen that everyone should 'do his own thing'; having chosen his staff and his research students with care, he then let them get on with it, however divergent from his own ideas their 'thing' might be. He had a sure eye for promising young psychologists: he recognised the exceptional capacity of the 22-year-old Kenneth Craik from the moment Craik arrived in Cambridge (and he never fully recovered from the latter's tragic sudden death in 1945).

As to Sir Frederic's humour, it permeated all his dealings with students and staff, and - at least at the receiving end - it lightened his heavy teaching load. I well recall his comment, made with characteristic wide slow smile, a propos the use of questionnaires in personality assessment, 'I don't know.....they may be all right.... they always seem to me to overestimate the self-knowledge of the Subject and to underestimate his sense of humour'. Now, some 35 years later, this still seems to be the swiftest and aptest summing-up of the position regarding such tests.

In those days, the head of department taught in more or less all fields; thought-processes, skills, learning, psycho-analysis, individual differences, social psychology and remembering. Bartlett is probably best known for his book on Remembering. This work has aroused many criticisms along the lines that it was vague, that it contradicted itself in places and that it did not offer a genuine theory of remembering. But - as the pendulum swings back from cybernetics and the rat-in-maze to the human being in his own right - Bartlett's achievement in this sphere will once again be appreciated; that he applied experimental method in a 'real-life' situation, while well aware of the difficulties of so doing; that he recognised and accepted the complexity of his subject-matter and, courageously, did not attempt to ignore or minimise it.

[1] Dr. Heim is in fact incorrect here. Dr. Boris Semeonoff was a founder member of the group. He was trained at Edinburgh.- not at Cambridge.

> This was one of Bartlett's most striking qualities as a psychologist and as a teacher. He understood people as well as ideas and logic and methodology. It was probably for this reason that, despite his many other commitments, he made himself available at all times to everybody who wished to discuss anything with him. It is clear that this human understanding is much harder to teach than is experimental method; yet Bartlett attempted both, with a considerable measure of success.
>
> Before becoming a full-time psychologist, he had already excelled in philosophy and anthropology - to say nothing of cricket, tennis and golf - and these many interests remained always with him. He retained the philosopher's love of clear, cogent thinking, the anthropologist's keen interest in people in their natural setting, and he never lost his delight in a neat catch, a well-placed volley or a 200-yard drive. Partly because of these interests and partly because of his pleasure in discussion on every conceivable topic, Bartlett maintained affectionate ties with members of every generation. Up to the time of his death at 82, he was equally at home with people of every age, including children and adolescents, and all enjoyed his companionship with equal delight".

As Buzzard himself (1971) wrote, "an apocrypha of stories giving insight into Bartlett's style could fill the pages of this journal and the rejects still another". Thus I do not intend to duplicate here the many affectionate accounts of Bartlett, the teacher, which appeared in his obituaries. To these accounts I would refer those interested.

Bartlett's teaching style was informal. According to some it was chaotic or even anarchic. Many, as we have seen, found his style inspiring. Others however found it almost as irritating as they did his way of running the department. This was,as Alice Heim has pointed out, often little more than anarchic. For staff he wanted to find good people and let them get on with it. Also although

Bartlett inspired loyalty and affection among his own pupils, to some outside the Cambridge setting he appeared somewhat cold, aloof and far too insular in his concerns.

Nevertheless what is in no doubt both from the literature (and the correspondence the current author has received) Bartlett did leave behind him a whole generation of eminent Cambridge psychologists who held him in the greatest esteem. As Donald Broadbent put it (1970b(2)) his death "robbed a vast number of middle aged psychologists of a figure they have always revered as the ideal master of their profession".

G. A Summary

This chapter has attempted to outline the activities of the Cambridge laboratory for most of the inter-war years. The main themes of this period were:

1) the growth of applied psychology in which Cambridge played a prominent part.
2) the growth of teaching activities at Cambridge which produced so many later emminent psychologists.

This chapter has also attempted to shed light on the somewhat enigmatic nature of Bartlett's contribution to British psychology. Broadbent (1970b op.cit.) pointed out that although Bartlett is held in great respect his main publication was published in 1932, was his only major work,

was fairly small and somewhat non-experimental and expressed in sometimes ambiguous language. Broadbent goes on to relate that Bartlett's reputation is far in excess of his actual published work and attempts to explain why this is. Broadbent's explanation is essentially in concordance with that presented above. Firstly Bartlett owes his reputation to his unique historical position and secondly to his personality.

His position writes Broadbent, enabled him to build up the Cambridge department along his own lines. Also at one time after the War the professors of Oxford, U.C. London, Cambridge and Edinburgh were all ex-pupils of Bartlett's. Regarding his personality Broadbent writes that, Bartlett was; a) always concerned with relating psychology to real life and applied situations; b) full of creative innovation and inspiration; and c) very good at stimulating research, mainly by supplying novel ideas.

Essentially then Bartlett's particular talents for teaching and administration seem to have been allowed full scope by his historical position. Hopefully this chapter has shed some light on exactly how it was that Bartlett's influence has permeated through so much of British psychology. Perhaps also we have somewhat solved the problem of why Bartlett's reputation outshines his substantive contribution.

CHAPTER 14

BARTLETT'S ACADEMIC WORK; 1922 - 1939

A. Introduction

Bartlett's work during this period falls fairly neatly into four categories. These are military, anthropological, industrial and that concerned with Remembering. The first three are dealt with below while the work leading up to Remembering is dealt with in the next chapter.

In fact in the period 1922 - 1939 the vast majority of Bartlett's academic effort was directed towards his major work Remembering. For example Bartlett's articles of 1924 (1), 1925 (2 and 3), 1927 (1), 1928 (2 and 3), were all preliminary work for this book. Towards the end of this period (e.g. Bartlett 1938, 1939) he began developing ideas for his other important book Thinking of 1957. This new emphasis on cognitive processes originated from Bartlett's anthropological work, which during this period slipped into the background. Conversely, during these years he showed an ever growing interest in applied psychology publishing many articles on military studies and rather fewer in the industrial field.

B. Military Work

As we have seen Bartlett's own involvement with military psychology began with the First World War hydrophone studies. These led on, naturally enough, to further studies such as his "A Note on Local Fatigue in the Auditory System". (1922). But Bartlett soon acquired a far more general interest in military psychology which eventually

bore fruit with his 1927 publication Psychology and the Soldier. In his preface Bartlett explains how the book came about;

> "During the Great War of 1914-1918 every important belligerent country called upon its psychologists for technical advice and assistance, and by common consent much of the work done proved of the greatest value. When the War was over and courses of Military Study were being re-organised in Cambridge, lectures dealing with psychology in relation to military problems were begun. They were due to the initiative of Dr. C. S. Myers, C.B.E., F.R.S., who was then Director of the Cambridge Psychological Laboratory, and who had been Official Psychologist to the British Expeditionary Force in France, and Lieut.-Col. L. H. Thornton, C.M.G., D.S.O., who was then Director of Military Studies at Cambridge. These lectures have been continued since that time, and I have been responsible for them for the last six years. The present book is a selection from the lectures delivered in connexion with this Course. It does not in any way pretend to be a complete treatment of the relations of Psychology to the soldier and to his life. It is merely an introduction to an important field of applied psychology which has in the past been unduly neglected."

The book has three main sections. The first deals with "Choosing and training the recruit," the second with "Leadership, discipline and morale" and the last with "Mental disorders of warfare". The work is really a record of the 'state of the art' in 1927. Thus it provides a valuable, historical record for those concerned with the growth of military psychology. However the book is worthy of note for other reasons too, as we shall see below.

Bartlett himself did not perform a great deal of original military research. Yet in Psychology and the Soldier

his interest and feeling for the subject are made abundantly clear. It is this book which displays to their best advantage Bartlett's qualities as a committee man - qualities which were recognised by the Armed Forces and other Government bodies both before and more especially during the Second World War. The book is not notable for any great innovation. It is however a clear and dispassionate account of the role of psychology in the forces, which outlines psychology's limitations as well as its possibilities.

The aim of the book is described by Bartlett in his "General Introduction". The advent of more complex machinery, be it for artillery, transport, tanks, aeroplanes, wireless, submarines or hydrophones, has created, he writes, new and sometimes unforeseen demands for human skilled performance. Furthermore, experience in the First World War had shown that performance of skilled operations was often required under conditions of both physical and psychological stress. Psychology could now, Bartlett claims, identify those qualities needed for specialised and stressful Army roles and then go some way at least towards selecting those men most likely to fill those roles efficiently.

This statement leads Bartlett into an interesting discussion of the nature of psychology. He writes (p.2)

> "Psychology has undergone a very great change during recent years. It is no longer merely a matter of the study, but of the laboratory and of daily life.

> Not long ago psychologists were chiefly concerned to give a detailed description of the contents of the human mind....(The psychologist) may call the result "psychology"; but however successful he may be in his attempt, his answers do not throw much light, and are not of course intended to throw much light upon his conduct...."

Consequently, Bartlett goes on, psychology has tended to become "more and more a study of how people behave and why they behave as they do". Indeed psychology may be defined, he writes, as a "systematic attempt to understand the conditions of human activity". Thus the psychologist is now "bound to be interested in the Army" because "he wants to find out why one man does this thing and another that, and why one man does this well and another the same thing ill".

This discussion is one of Bartlett's first attempts to formulate a scheme for psychology that is both coherent and practical. He states (p. 4) that psychology was now mainly about "the conditions of all forms of human and animal behaviour". But he is quick to qualify this with the now classic Bartlettian warning;

> "But we cannot, by studying the external circumstances and by observing, however accurately, the bodily attitudes, say exactly what they are thinking about. Yet their thinking must be regarded as a form of activity. A dream is a form of activity. Having an image is a form of activity. There are in fact many forms or instances of activity which only the person who displays them can tell us about. Not only so, but it is evident that what a person thinks may have no small influence upon some of his more obvious forms of behaviour both then and later. For much of our evidence concerning the conditions of activity we are driven to go to the person whose behaviour we are studying and to rely upon what he can tell us. Whether this must render

> many of the psychological formulations of the conditions of activity finally uncertain, since a man is very liable to error in the matter of his own inner life, I do not now propose to discuss. The fact must be admitted, and instead of discussing its exact significance in general terms, it is better to see what psychology, in spite of this difficulty, is able to accomplish".

Having established this eclectic, pragmatic standpoint, which probably owed much to Myers' influence, Bartlett goes on to present exactly what this sort of psychology can offer;

The section "Choosing and training the recruit" firstly deals with the new need for selection. New forms of transport required "special visual skill," good manual dexterity and muscular control and "the sort of imagery that is able to cope with mechanical problems". New systems for signalling needed keen vision, good colour discrimination and sometimes unusually acute acoustic abilities. But all these abilities will be useless writes Bartlett without the necessary interest and motivation for, "interests are less easily made to order than has been popularly or officially supposed".

Luckily, Bartlett continues, "the more technical laboratory type of psychology can help" with these problems. He then goes on to describe the standard psychophysical methods for measuring the "special senses". He emphasises the far greater sensitivity of these new psychological methods over more traditional medical approaches. Not surprisingly Bartlett pays much attention

to the overall procedure used for hydrophone operator selection. This involved measurement of intelligence, ordinary auditory acuity and special capacities to recognise rhythm and pitch.

This section goes on to review the state of testing for intelligence and special abilities before dealing with the training of "bodily skills" and the problem of fatigue. This first part of the book is notable for the enthusiasm with which Bartlett propounds the application of laboratory techniques.

The remainder of the book is of less interest here. Part 2 covers "Leadership, Discipline and Morale". It does so however in a way which all too clearly reflects the then rather impoverished state of British social psychology. His treatment is almost totally descriptive, for Bartlett seemed to regard the problems as being susceptible to insight - rather in the same way as clinical problems were. The most valuable section here is probably his warning that officers should all be aware of the need for good morale and effective leadership. (Bartlett's approach to social psychology is dealt with further below). Part 3 of the book is essentially a review of "shell shock" and need detain us no longer, (see Chapter Ten for details).

Bartlett continued to give courses in military psychology until 1939. However apart from the occasional special invitation lecture his developing thoughts have

not been recorded. He was invited to lecture to the Royal Engineers (1929, 1931) and prepared a series of lectures for the Royal Air Force in 1937. Other than this his interest in military psychology lay dormant until the War.

Bartlett was never in any doubt of the importance of military psychology. In 1969 he wrote;

> "The fact that the years 1914-1952 included the two great wars with Germany was of outstanding significance for experimental psychology. It established an outlook directed far beyond College or University, a conviction that psychology has a vital part to play in the invention, control and application of technological advance of many kinds.....Much of this became clear to me almost as soon as I took over the direction of experiment in Myers' new laboratory in 1914".

The first War in particular he writes (op. cit),

> "had two vastly important implications. The first was that it strongly re-inforced the view that experimental psychologists must be prepared to work together with experts in other, perhaps many other, fields. The second was that experimental psychology might well itself contribute fundamentally to developing technological knowledge and technical practice".

Military psychology played an even greater role in the Second World War. Bartlett's laboratory in particular developed invaluable studies of skill, simulation of complex motor performance and of course vigilance. We cannot deal here with these interesting and most important developments, except, in passing, in our Postscript Part 5.

C. Bartlett's Social Psychological and Anthropological Work

Bartlett's contribution to these fields was mainly theoretical. He never undertook any "fact finding" expeditions in the manner of Rivers but he did attempt to promote the theoretical tradition of his mentor.

This attempt has its best exposition in Bartlett's main anthropological work, his Psychology and Primitive Culture of 1923. The book is notable not only for its contribution to an ongoing theoretical debate but also as a sign of what Harris and Zangwill call Bartlett's own "unrealised anthropological vocation".

Bartlett's book is only comprehensible within the context of the protracted, intricate and sometimes profound debate between anthropologists, sociologists and psychologists in the first quarter of this century.

Peters (1953) writes of this tri-disciplinary conflict thus;

> "Broadly speaking it could be said that the first reaction of psychologists to the accumulating researches of social scientists was to attempt explanations of them in terms of the invariable characteristics of individuals. In fact ever since Aristotle's conception of man as "by nature" a social and political animal it had usually been assumed that social organisation was a reflection of man's needs and requirements". (p. 709)

Broadly this view had been followed by, Peters claims, Hume and Hobbes. When psychology became established there was nothing, he writes "very novel in the psychological approach to social phenomena which became so popular at the end of the nineteenth century and which lasted for the first quarter of the twentieth".

Wundt's "Folk Psychology", Peters continues, was an attempt to interpret ethnological and historical material from a psychological viewpoint as was Le Bon's Psychology of the Crowd. Galton and later Wilfred Trotter also added to these rather grandiose psychological forays into the nature and history of society. But, as Peters states;

> "The psychologists ..... who went in for this type of nativistic explanation of social phenomena in a big way were McDougall and Freud".

Freud's ideas are too well known to bear repetition here. Essentially of course he saw social organisation as a system of restraints which had developed to hold in check the basic instincts of humans. Modern society and its development from primitive cultures could all be explained using the Freudian psychological model of man. We have seen in Part 3 the enormous impact which Freud had in Britain from the 1914-1918 War onwards. However as Hearnshaw (1964) has pointed out McDougall's competing ideas also enjoyed an "enormous vogue" from 1908 onwards throughout the inter-war years. McDougall's theory was of course a development par excellence in psychology of the biological concept of instinct. He maintained that all men were born with similar instinctive tendencies and that all social processes occurred as the result of the interaction of these tendencies. Like Freud's, McDougall's theory was a form of psychological reductionism.

The reaction to these views, to rather oversimplify, came from two directions. Firstly the ideas of Charles

Cooley, Ginsberg and G. H. Mead gained ground, especially in America. Cooley's and Mead's "looking glass self" and "symbolic interactionism" were especially important. They stressed the importance of social factors in the development of human personality. They stressed that the social influences on humans were so great as to almost totally obscure any instinctive tendencies which might be present. Secondly, and of more concern to us here, in Britain the anthropologist Radcliffe-Brown rallied his discipline towards the "functional" approach. He rejected the need for any psychological interpretation. For, he argued, it was sufficient for the social anthropologist to study the functions of institutions and customs at a comparative sociological level. The anthropologist's task was not, he argued, to speculate on the pre-history or psychological mechanisms of, for instance, totemism, but to look at the social significance of the phenomenon. (For further discussion of these points see Hearnshaw op. cit pp. 234-236 and Peters op. cit. pp. 709-714).

Rivers' standpoint on this issue (see Chapter 10) was that psychology and anthropology should become thoroughly integrated. For he considered that psychology could offer insights into the origins of social customs, rites and religions while similarly anthropology could offer insights into an individual's and a society's beliefs, conceptualisations, fantasies and habitual symbolisations. However it should be stated that in his later

years, as we have seen, Rivers was heavily, though conditionally, influenced by Freud. It is thus probably true to say that in the end he veered considerably towards what Peters has called the "nativistic" approach to anthropology.

Bartlett's basic framework for Psychology and Primitive Culture owes much to both McDougall and Rivers.

From McDougall comes Bartlett's acceptance as axiomatic that;

> "The underlying psychological mechanisms.... remain much the same at all stages of social development.....Some of these - the instinctive tendencies - are common to all people, others - the individual difference tendencies - differentiate one person from the other".

Bartlett's book was really an attempt to delineate the ground rules for the social psychological study of primitive societies. This was a valuable exercise, he claimed, because the social structure of primitive cultures was far simpler and thus the basic principles behind its operation would surely stand out. He wrote; "the psychological study of primitive culture forms the best introduction to the psychology of contemporary social life". Thus Bartlett clearly believed that the social processes indulged in by primitive peoples were essentially similar to those of modern society. Support for this point of view came from the work of McDougall. However it was backed up by some of Bartlett's own findings, with regard to "conventionalisation". (This topic is covered in the next chapter - essentially Bartlett produced some evidence to suggest that primitive people and societies handled symbols in the same manner as modern people).

However although Bartlett's model of social man was largely from McDougall he applied it in rather a differ- way. For instance the focus of his work was to isolate observable determinants of behaviour. He stressed repeatedly that he was interested in the concrete, immediate cause of behaviour rather than "the absolute, pre-social origin of social facts". This aspect of the work follows Rivers in that it stresses observation and accepts social facts as determinants of behaviour without insisting on the ultimate psychological explanation of all social events.

The book is also interesting for its rejection of the French anthropologist Levy-Bruhl's conception of the pre-logical nature of primitive thought. Bartlett (see pages 282-285) attacks Levy-Bruhl on two fronts. Firstly he claims that basic social processes are the same in modern as in primitive society. The differences are not qualitative he claims, in that historical and environmental influences affect the degree to which social behaviours will appear. Similarly he again quotes his work on conventionalisation to support the idea that the primitive mind operates in an essentially similar way to the modern version.

Essentially then, Bartlett's book was, as Zangwill (1968) puts it, an argument for the analysis of group processes in terms of a small number of basic human tendencies. Prominent among these were comradeship, assertiveness and submissiveness. Bartlett seemed to believe that these underlying tendencies would be found to be the same in all societies and thus that a more basic, primitive society would produce useful insights into a more complex modern one.

Bartlett's "Psychological Methods and Anthropological Problems" of 1937 looked at the problem of culture fairness in cross cultural psychological testing. As we shall see in Chapter 16 Bartlett always had something of a disinclination towards psychological testing. In this articlehe argues that intelligence tests had no adequate theoretical backing and furthermore that when applied without any reference to the culture under study could give seriously misleading results. Bartlett's Maudsley Lecture (1947) repeated these arguments.

In 1939 (Study of Society) and 1943 (Anthropology in Reconstruction) Bartlett returned to the theme of the relation of social psychology and anthropology. Largely pursuing Rivers' views he emphasised the extent of their common ground, the possibilities of cross fertilization and the need for more precise research techniques in both disciplines.

Bartlett's interest in the methodology of anthropology and social psychology was fairly considerable. It stemmed of course from his adolescence when, under the influence of Rivers, he almost pursued a career in anthropology. However his legacy to this area was not of any great importance.

This was partly because British social psychology in general was notably slow to develop. As Hearnshaw (1964 p. 234) has put it in the 1920's and 1930's the discipline was hampered by its "sidetracking....by McDougall into the discussion of instincts and the group mind".

Furthermore although McDougall was sweepingly popular at this time his theory was based on the rather limiting notion of instinct. As Hearnshaw puts it (op. cit. p. 90)

> "For hormic psychologists of the McDougall type the classification of the major instincts has always been a troublesome problem. Not only did other writers disagree with McDougall, but McDougall could not agree with himself, and each new book continued a somewhat different list".

Suffice it to say here then Bartlett's contribution to these areas is probably the least noteworthy of his efforts. His ideas had little impact and being so much tied to the now outmoded tradition of McDougall have little lasting value.

D. Industrial Studies

Indubitably Bartlett, at least before 1939, contributed far more to industrial psychology as an administrator and sponsor of research than as a researcher himself. He only produced four industrial publications in this period. "The Social Psychology of Leadership" of 1926 bears a distinct similarity to the views already discussed, of social psychology in Psychology and the Soldier and Psychology and Primitive Culture. "Some Observations on Recent Psychological Experiments on Visual and Tactile Judgements of Wool Fibres" of 1937 is rather too particular to be of interest here.

In 1932, with the help of one K. G. Pollock, Bartlett conducted some experiments on the disturbing influence of noise at the request of the I.H.R.B. In 1934 Bartlett was asked to lecture on The Problem of Noise for the N.I.I.P. under which title the lectures soon appeared in print. The book was merely a review of current research on the problem. It is however notable for being the first appearance in print of Bartlett's more informal style of writing. The book begins, "Everybody agrees that a lot of noise is a bad thing". This ranks second only to Bartlett's entrance to Psychology and the Soldier with, "If the Duke of Wellington had been accused of being a psychologist, his reply would probably have been brief but very emphatic".

CHAPTER 15

"RETURN TO REMEMBERING"

A. Introduction

This chapter is intended to discuss Bartlett's major work Remembering and to examine in some detail the importance of this rather out of the ordinary psychological text. The book is unusual, especially by modern standards, for its lack of experimental precision, for its subjective, almost anecdotal, evidence and for its often ambiguous, inexact language. Yet, despite all this, Bartlett's findings are still accepted, almost without question, to this very day. Furthermore hardly a book on memory has emerged, since 1932, which has failed to mention Bartlett's work. Remembering has also found its way into nearly every general text book written since 1932. Strangely enough, Remembering most certainly wields just as much influence today as it did in the 1930's. It is still just as frequently quoted as it ever was and is selling (now in paperback form), to the undergraduates of the 1970's who still, by and large, replicate Bartlett's findings in their laboratory classes.

Remembering has indeed become something of a ubiquitous classic. Baddeley in the most recent major British publication on the subject divides the study of memory into two traditions - the "Ebbinghaus" and the "Bartlett". He also comments upon the dogged persistence of Bartlett's ideas and on their current revival. Summing up his book Baddeley writes, (1976);

> "We began with a discussion of the two traditions in the study of memory. The first, originating with Ebbinghaus, tends to emphasise the need to limit and control the complexities of human memory in order to make the problem tractable. The second, represented by the approach of Bartlett, prefers to accept the complexity of studying memory in more naturalistic situations rather than risking the exclusion of the subject's search for meaning, which it regards as central to the process of remembering. As we have seen, in the 1950's the Ebbinghaus approach was clearly dominant, as represented by functionalist and S-R associationist views of memory. In the 1960's the balance began to swing more in the direction of the Bartlett tradition, with the emphasis on less rigidly controlled techniques, such as free recall, and on increasingly complex information - processing interpretations of experimental results. It is probably true to say by the mid-1970's that the pendulum has swung very strongly toward the Bartlett rather than the Ebbinghaus approach. This is true both figuratively, in the emphasis on richer experimental material and more complex and flexible theoretical interpretations, and also literally, in the sense that Bartlett's work is being cited very much more frequently than in the 1950's and many of the experiments being performed and the interpretations proposed are very close in spirit to Bartlett's own work of 40 years ago".

Indeed the phenomenon of the Bartlett revival is now well established. Most of the memory work of the 1930's now gathers dust on ill frequented library shelves, while Remembering has been picked up, dusted off, and rediscovered by a new generation of "Cognitive Psychologists". It is one of the aims of this chapter to describe and explain this rather unusual turn of events.

However it is not proposed here to present a lengthy recapitulation of Bartlett's book. The main themes and

findings are well known and accounts are available in Zangwill (1972), Oldfield and Zangwill (1942) and Baddeley (1976). Special emphasis is however given below to the origins of Bartlett's thinking - for three reasons. Firstly the preceding sections of this thesis hopefully, provide the detailed backdrop necessary for such a project. Secondly, the current author is able to refer not only to Bartlett's autobiography but also to his unpublished article "Return to Remembering". This article attempted to rebut the various criticisms of Remembering which had accumulated over the years. But the article also provides Bartlett's own most detailed account not only of the major influences on him but also of his intentions in writing the book.(1) Thirdly the previous accounts of the origins of Remembering have been a little cursory.

The account of Remembering given below is largely chronological. The work was inspired by the thinking of Ward and Rivers who interested Bartlett in the constructive activity of perception and memory and in "con ventionalisation" respectively. Section B presents Bartlett's development of these ideas in his 1916 fellowship dissertation. The major development between the dissertation and the final book was not in the data but in the theorising. The main reason for this was Bartlett's incorporation of Head's notion of schema (see Section C). Section D then describes the other aspects

(1) As far as I am aware no previous work has dealt with "Return to Remembering".

of the book itself while Section F reviews the book in the light of modern theories of memory. Section E presents Bartlett's own "Return to Remembering". The impact and importance of the book is related in Section G.

B. The Fellowship Dissertation and Other Early Experiments

(All quotations in this section, unless otherwise indicated, are from Bartlett's fellowship dissertation).

The First Experiments

> "On a brilliant afternoon in May 1913 the present Laboratory of Experimental Psychology in the University of Cambridge was formally opened. Dr. C. S. Myers.... invited me to take part in the Demonstrations which he had arranged. Accordingly, for several hours, I sat in a darkened room exposing geometrical forms, pictures and various optical illusions to the brief examination of a long string of visitors. This was the beginning of the present book. The interpretations by my observers of the figures which were placed before them were as various as they were attractive. It was clear that the course of normal visual perception may be determined by a very large number of different factors. It seemed probable that a carefully arranged experimental study might serve to disentangle many of these factors and to demonstrate their functions. Encouraged by others, I set to work at once. But it very speedily became evident that an examination of normal perceptual process leads directly and inevitably to an investigation of related mental processes, and in particular to a study of imagery and of recall";

thus wrote Bartlett in his introduction to Remembering.

The work which he immediately undertook appeared in Bartlett 1916 (1) but was more fully developed in his

fellowship dissertation "Transformations Arising From Repeated Representation; A Contribution Towards the Experimental Study of Conventionalisation". But we must deal first with Bartlett's first published paper in psychology, "An Experimental Study of Some Problems in Perceiving and Imagery". This paper marked the very first step towards Remembering and was reproduced, in condensed form, as Chapter 2 of the final book.

His rationale, reported in Remembering (p. 14) was that;

> "a great amount of what goes on under the name of perception is in the wide sense of the term recall..... (the observer) fills up the gaps of his perception by the aid of what he has experienced before in similar situations..... He may do this without being in the least aware that he is either supplementing or falsifying the data of perception".

Thus, Bartlett goes on, there is a good prima facie case for the close intermingling of perception and recall.

The experiments to investigate this effect were very simple. Four groups of pictures on cards were drawn up. The groups varied from simple geometric designs through more complex designs to a drawing of a real scene (such as a pointing hand, or a gate in a wall). Thirty subjects were used. Each card was presented for an interval between $\frac{1}{15}$ and $\frac{1}{4}$ of a second. Repetition of presentation was allowed if requested. Immediately afterwards subjects were asked to either draw or describe what they had seen. Bartlett noted a marked tendency

by subjects to report and draw the presence of familiar objects when none was present. Also naming of ambiguous shapes often took place which seemed to satisfy subjects but also tended to change their drawings towards the named object. The last group of "real life" drawings was greeted "with relief" - after the three groups of geometric designs - except by those used to dealing with diagrams. These effects led Bartlett towards his famous notion of "effort after meaning" described in the next section.

Bartlett's other main conclusions were three-fold:

1. "Dominant details" often appeared to guide response. These details varied from gaps and simple spatial relations in simple figures to a "plan of construction" for more complex ones. But generally; "These dominant details are a kind of nucleus about which the rest cluster. They set the stage for remembering.
2. There is usually a search for "analogical material" which matches the perception and then affects the recall of it.
3. Significant individual differences in reporting occurred. Some were rash, leaping to conclusions while others were persistently more cautious.
4. That much of what subjects reported actually seeing was, in fact, inferred.

In Remembering (p 33) Bartlett sums up these experiments by stating:

> "Nobody who reflects upon how variously determined are the processes and content of perception will be prone to give a careless allegiance to the theory of lifeless, fixed and unchangeable memory traces".

But another upshot of these experiments was Bartlett's increasing realisation of the importance of imagery. Often subjects would report some vivid visual image[1] which would dominate and/or direct perception. (One subject even gave up on one picture because it always evoked a similar, completely dominating, image which effectively "wiped out" his capacity to recall the picture in question).

Bartlett investigated imagery by presenting his subjects with thirty six coloured ink blots on postcards. He told them to "make what you can of them, as you sometimes find shapes for clouds, or see faces in a fire". Bartlett again drew four main conclusions:

1. There was an enormous variety of images reported.
2. Some 72 per cent of all reports were of living animals.
3. Subjects'interests or occupation strongly influenced the nature of their reports.
4. Once a specific response occurred there was a marked tendency for it to persist - often to a subject's considerable annoyance.

(1) Myers at first treated these findings with total incredulity as he was almost totally without any visual imagery.

From both these sets of experiments Bartlett claimed evidence for his growing beliefs that all cognitive processes from perceiving to valuing to remembering were closely interlinked; that such processes were active and characterised by an "effort after meaning"; and lastly that large individual differences occurred in these processes.

But now he had to seek some empirical evidence for this embryonic theorising.

An Account of the Dissertation

This evidence began to emerge in Bartlett's 1916 fellowship dissertation entitled "Transformations Arising From Repeated Representation; A Contribution Towards the Experimental Study of Conventionalisation". This dissertation contains the large majority of Bartlett's now famous experimental methods as well as a development of the concept of "effort after meaning".

(i) The Introduction and "Effort after Meaning".

"Faces", says R. L. Stevenson,

> "have a trick of growing more and more spiritualised and abstract in the memory until nothing remains of them but a look, a haunting expression, just that secret quality in a face that is apt to slip out under the cunningest painter's touch. He is giving an illustration of something which almost everyone must have noticed, but which hardly anybody has systematically studied".

These are the first few sentences of Bartlett's dissertation. In them he has indicated two of the work's main themes. Firstly he is outlining his desire to pursue a

more life-like line of enquiry than that of previous workers, notably Ebbinghaus, into problems of perception and memory. Bartlett returned to this theme in Chapter 1 of Remembering (see below). Secondly Bartlett is emphasing his desire to study the general problem of the way man handles his representation of the external world and the specific problem of how certain images or representations are apt to change.

The dissertation, Bartlett continues, develops his experiments on perceiving and imagery (Bartlett 1916 (1)). "These" he writes,

> "made it clear that very constantly, and particularly when material presented is complex or ambiguous in structure imagery comes to the aid of perceiving. That which is apprehended is filled up by that which is imaged, though frequently, of course the subject does not in the least suspect that he is carrying out some "filling up" process".

Here Bartlett introduces his main theoretical idea, namely, that of "effort after meaning". This concept, which remained virtually unchanged, is fundamental to all the work leading to Remembering. The notion refers to what Bartlett saw as man's basic requirement to actively make some sense of his world. Zangwill (1972) accurately describes the concept as, "an unwitting attempt to match whatever is presented to some pre-existing setting or scheme and which does not necessarily imply felt effort or strain on the part of the percipient". The notion

is so central that we must dwell on Bartlett's own words on the topic. In the dissertation he writes:

> "The expression was employed to indicate a very constant general tendency, on the part of a subject, to link on that which is now being experienced with something that has been experienced already, so that a present object is given a setting".

Later, in Remembering he writes of these original 1916 experiments:

> "The connecting of the given pattern with a special setting is obviously an active process, for, speaking in an abstract sense, the setting used is only one of a large number, any of which might be brought into play. But though it is active it is not conscious, for the observer is not aware of a search and a subsequent match. I shall call this fundamental process of connecting a given pattern with some setting or scheme: effort after meaning". (Bartlett's emphasis: p. 20).

Later, on page 44, he writes;

> "Because this task factor is always present, it is fitting to speak of every human cognitive reaction - perceiving, imagery, remembering, thinking and reasoning - as an effort after meaning".

Such then were Bartlett's aims and main theoretical orientation for the dissertation. Returning to his introduction Bartlett goes on to relate his debt to the French psychologist Jean Philippe. From Philippe's paper "Sur les Transformations de Nos Images Mentales" (Philippe 1897), Bartlett writes that he became interested in the role of "imagery in mental life" and especially in conventionalisation.(1) Philippe, just as Bartlett did, expressed

(1) This term discussed later is defined by Zangwill (1972) as: "The process whereby a cultural element, such as a work of art, transmitted by diffusion from one society to another gradually loses its representational significance and takes on a formal, conventional and non-representational character".

a dislike of the notion, perpetuated by Ebbinghaus, of man as a passive recipient of sensory information. Using imagery as his main case Philippe wrote; "L'image est mobile et vivante et soumise a de perpetuels changements sous l'encessante action de nos sentiments ou de nos idees".

Philippe reached these conclusions on the basis of experiments using the repeated reproduction(1) of common objects both visually and tactilely. He noted three main kinds of change. The image could disappear, details could fall out or grow to dominate the image or, lastly, generalisation to some pre-existing image of an object could occur.

In general terms, Bartlett wrote, Philippe showed that there were, "side by side with lifeless, fixed memories, live mental images that are often and constantly changing".

Bartlett ends his introduction by writing that in contrast with his own previous work, and with Philippe's, he is now pushing forward "to a somewhat higher level and shall deal with ..... the stages by which we pass from perceiving to thinking". To do this he proposes, at the end of his Introduction, three key avenues of enquiry into the functions of images:

1. What is the stock of images?
2. How do these arise and develop?
3. How do they change and disappear?

(1) Details of this method appear below.

(ii) "Imagery And Having An Idea"

In this section Bartlett clarifies his usage of the word image and makes his case for importing psychological methods into the study of the higher mental processes - an area then still very much in the possession of the philosophers - at least in Britain.

Characteristically, of the Wardian tradition, Bartlett argues that an idea is more than a passive mental content. An idea he writes, "is a mode of consciousness which in the scope of its reference goes beyond what is directly present to our senses". Furthermore "the content (of an act) has always a reference beyond the present, and that is what we call an idea".

Bartlett goes on to endorse the views of Brentano regarding "transitive reference - or the unique pointing of one thing to another".

> "Such pointing", he continues, "is precisely what has here been called the function of effort after meaning. If then we may admit that the transitive reference may be found over the whole range of mental life - and in this respect Brentano and his followers certainly appear to be correct - it would follow that there must indeed be an ideational element in every mental content, and that this is to be identified with what is here meant by meaning.....(Thus) imagery is to be treated as one of the ways in which effort after meaning finds expression".

Having thus outlined his own generally Wardian position (which as we have noted earlier (Chapter 5) bears a "distinct family resemblance" to that of Brentano). Bartlett goes on to say that he is only interested in how imagery arises and what conditions influence its nature and

functions. This problem is "amenable to scientific study," he claims. Of the philosophical problem of what an image or idea is, he writes, "what they are I do not know and at present I do not care".

(iii) Conventionalisation; the Influence of Rivers

Thus far Bartlett's dissertation seems to be concerned purely with the role of imagery in what Bartlett conceived of as man's integrated cognitive system. Now, as we know, one of the distinctive features of Remembering was that it discussed social determinants of cognitive behaviour. These were mainly introduced through Bartlett's interest in conventionalisation. This interest, in turn, derived almost entirely from Bartlett's contact with Rivers.

Earlier, in Chapter 10, we have noted that Rivers was concerned to unify ethnology and psychology. One of the strands of evidence for this unification could be found in the study of conventionalisation. This process occurred "When a technique, a custom, or an institution is adopted into one group from another" and its study deals with the way, "the recipient group always works it into a pattern which is distinctive of itself," (Bartlett 1932 p. 268).

As we have seen, one of Rivers' main interests was the study of culture contact. His demolition of Bastian's "psychic unity" theory depended very much on the assembly of evidence supporting the possibility of diffusion and

and transmission of techniques, customs, institutions, beliefs, ceremonies, symbolic art and so on. Thus if evidence was forthcoming that symbols could become distorted and changed as individuals processed them in a similar fashion to the distortions and changes observed in culture contact, Rivers could claim support for two of his pet ideas. Firstly psychological evidence would have shed light on an ethnological problem and secondly a psychological mechanism for the process of cultural diffusion would have been found.

It is hardly surprising then that Rivers should prod Bartlett in the direction of this fellowship dissertation. Bartlett writes in his section "conventionalisation," "it is this process so far as it takes place in a series of reproductions given by the individual that will mainly occupy this inquiry". He goes on to explain that he sees his current studies of man's activity with symbols as a stepping stone on the road to the psychology of thinking;

> "It is because I am convinced that the study of the art of thinking must be preferred by a consideration of many antecedent circumstances in the mental life that I am gradually approaching the final problem".

Bartlett retained these views, first expressed here in 1916, until the end of his days. Indeed his own life in psychology, progressing as it did through "Some Problems of Perceiving and Imagery" (1916 (1)) to Remembering (1932) and to Thinking (1958), exemplified this point of view.

(iv) Methods and Conclusions

In these last two sections Bartlett moved on to discuss the results of his four methods of investigation namely Description, Repeated Reproduction, Picture Writing and Serial Reproduction. Both these methods and the original results reappear largely unchanged in Remembering and hence are related in some detail below.

1. Description

Twenty subjects were presented with five drawings of the faces of five military officers. Each face was presented for ten seconds and subjects were told they would have to be able to describe the faces later. After periods of half an hour, a week and then sometimes after another week, subjects were asked to describe the faces in the correct order. If necessary, subjects were asked questions on "angle of regard," particular details, details from the other faces and details not in any of the faces.

This method "was a preliminary one, expected rather to suggest problems and indicate possible clues to their solution than itself to produce anything final" (Remembering p 47).

2. Repeated Reproduction

This method, wrote Bartlett, followed "almost exactly the plan of investigation adopted by Philippe". However Philippe worked almost entirely with children, his experiments were far shorter and he used common objects not stories or pictures, as did Bartlett. Zangwill (1972) had no hesitation in writing that; "In my view Bartlett's

method ..... should be regarded not only as original but as the source of much that is valuable in his work".

In this method subjects were presented with an original stimulus of either a short story of about 300 words or a drawing. They were able to read the story through twice or inspect the drawing for two periods of ten seconds. After fifteen minutes subjects were asked to write down the story or to draw the picture. Further reproductions were obtained "at intervals as opportunity offered".

"By using this method," writes Bartlett (op. cit. p. 63), "I hoped to find something of the common types of change introduced by normal individuals into remembered material with increasing lapse of time". He goes on to relate his intention to "check the progressive nature of.... transformations as they occur". He also points out the similarity of this process with that involved in the transmission of rumour and legend. The original dissertation experiments involved the use of both stories (the most famous being the "War of the Ghosts") and drawings. However only the former reappear in Remembering as the processes noted for drawings were almost identical to those Bartlett noted using the method of Serial Reproduction.

3. Picture Writing

This method involved the use of three different series of eighty arbitrary signs. Each sign was connected to a word and both word and sign were written on a postcard. Subjects were given between seven and fifteen minutes to inspect their series of cards and were told

that they would be asked to reproduce the relevant sign given the words as stimuli. In fact after fifteen minutes and then at intervals of about two weeks, and sometimes again after longer periods,[1] a story was read to the subjects. The subjects then had to draw the sign whenever the connected word occurred in the story.

Twenty two subjects were used, involving a total of 1,200 words. This method was intended to illustrate fairly directly the process of conventionalisation of symbols in individuals. Bartlett wrote in Remembering (p 95)

> "Conventionalisations are produced by a combination of innumerable small changes introduced by a large number of individuals; and it is not fantastic to suppose that there may be a parallel between them and the development, in the course of individual recall, of relatively fixed and stereotyped modes of representation or of reaction".

4. Serial Reproduction

In the final book Bartlett devotes as much space to this method as he does to the remaining three put together. As Zangwill (1972) put it;

> "He clearly regarded it as having much to contribute to our understanding not only of the influence of cultural factors on memory but also of communication within and between widely different social groups".

The method was simply an analogue of the spread of a rumour. Both stories (similar to those used in Repeated Reproduction) and pictures (line drawings of an owl, a cat

(1) One subject was tested after nine months.

and an abstract representation of a face) were used. Subjects either read the story twice at their own speed or were allowed to inspect the drawing for two to three minutes. After fifteen to thirty minutes subjects were asked to reproduce the story or drawing. This reproduction was then used as the stimulus for the next subject in the chain. The numbers in the chain varied from eight to twenty.

This method had similar objectives to Repeated Reproduction its main advantages being firstly that a greater number of changes in representation could be observed and that a closer analogue to social transmission could be obtained.

The results of all these experiments are notoriously hard to summarise. Bartlett's presentation of his data depended on him selecting what he regarded as typical examples of responses which he then used to illustrate his main conclusions. As Broadbent (1970(2)) put it; "Such a method places an enormous strain upon the ability of the experimenter to grasp what is truly significant".

Zangwill (1972) however has attempted this task and his account of the main findings is one with which the current author would heartily concur:

> "The most outstanding finding however is the extraordinarily high proportion of inaccuracy occurring in all reproductions, not excluding the first, and the fact that the subjects appeared to be totally unaware of the extent of their inaccuracy. These features appear most strikingly in the results of the experiments on the repeated and serial reproduction of stories, from which it becomes clear that inaccuracy results not only from omission and condensation, as might have been

> expected, but from radical transformation of the original material. Style is poorly conveyed, irrelevant material discarded, and what is retained undergoes marked, persistent and sometimes progressive rationalization. These changes are particularly apparent in the case of stories originating in alien cultures, in which the whole of the narrative may be recast in a form compatible with the subject's own cultural background and social conventions. They are also very striking in the experiments on serial reproduction of pictures. The various forms which transformation in memory may take were analysed by Bartlett with great perceptiveness and skill and some of the factors upon which they depend, both individual and social, discussed at considerable length. His main conclusion is that remembering can in no sense be regarded as the mere revival of earlier experience; it is a process of active reconstruction, much of it based on factors of general impression and attitude, together with the reinstatement of a small amount of critical detail. In his own words, recall is far more decisively an affair of construction than one of mere reproduction".

Bartlett's original conclusions (from 1916) are three fold:

1. Interrelated factors from the affective, attitudinal, cognitive and conative domains entered into the process of reproduction. These were "imageless processes" which Bartlett pointed out were similar to those reported by the Wurzburg School.
2. These factors could act in various modes producing; "Confusion, condensation, transport, invention, omission, simplification and elaboration".
3. These studies seemed to produce evidence for the existence of psychological mechanisms which could produce the ethnological phenomenon of conventionalisation.

The original theoretical interpretation of these results, which does not concern us greatly here, owed much to the notion of Einstellung propounded by Betz. But Remembering, in its final version, owes a great deal of its lasting value to the totally new theoretical reformulation which Bartlett gave to this data. This is largely because the 1932 version marked a distinct break not only from a largely introspective psychology but also from the influence of Germany. To this theoretical reformulation we turn in the next section - the original theory need detain us no further.

C. RememberingI: the Theory and the Concept of 'Schema'.

Definition of Schema

Two other major articles preceded the publication of Remembering. "The Functions of Images," in 1921, and "The Relevance of Visual Imagery to the Process of Thinking," in 1927, both discussed the role of imagery in cognitive processes on the basis of Bartlett's original experiments especially those involving Description. Their implications are described below. But we turn here to a discussion of the major innovation of Remembering - Bartlett's reformulation of his data using the concept of 'schema'.

Bartlett's own definition of the concept is (Remembering p 201); "an active organisation of past reactions, or of past experiences, which must always be supposed to be operating in any well adapted organic response".

Thus all new experience has to be assimilated within an existing schema. In this process it will be organised according to the nature of the pre-existing schema. Broadbent (1970(2) describes schema thus:

> "He (Bartlett) regarded all experience.... as stored in the brain in a condensed form.(1) This condensation was such as to lose detailed information about the series of past events, but merely to preserve a present state representing the current position: and each new event was perceived in the light of the appropriate schema. Thus it would be seen both selectively and in accordance with experience".

The organisation of schemata was governed by several factors as well as chronology. These others included "attitudes, orientation, appetitive and instinctive tendencies and interests as our active, organising factors," (Remembering p 307). Emphasising individual differences Bartlett continues (op. cit. p 309);

> "All that we can say for certain is that the mechanism of adult human remembering demands an organisation of 'schemata' which depends upon an interplay of appetites, instincts, interests and ideals peculiar to any given subject".

In general terms Bartlett saw the development of schemata as functional and evolutionarily advantageous. He propounds this view in his discussion "A Theory of Remembering" (op. cit. p 205) and repeats it in "Return to Remembering". As organisms develop he argues;

(1) Bartlett (1968) however is quite emphatic that he did not wish to rule out the possibility of "total storage" somewhere in the central nervous system. (See Section F).

> "...the special sense avenues increase in number and range, and concurrently there is an increase in number and variety of reactions... All this growth and complexity makes circularity of reaction, mere rote recapitulation and habit behaviour often both wasteful and inefficient. A new incoming impulse must become not merely a cue setting up a series of reactions all carried out in a fixed temporal order but a stimulus which enables us to go direct to that portion of the organised setting of past responses which is most relevant to the needs of the moment".

There is only one solution to this problem Bartlett, (tentatively) suggests. The organism must develop a complex 'schematic' organisation of experience. For practised skills and habitual or routine behaviour schematic determination of response will be adequate. But for 'higher' more complex functions such as complex recall, thinking, imagining and so on "an organism has somehow to acquire the capacity to turn round on its own schemata and construct them afresh. This is a crucial step in organic development. It is where and why consciousness comes in; it is what gives consciousness its most prominent function".

Thus Bartlett uses the term schema to describe the whole gamut of man's activity in the representation of the external world and even the evolution of consciousness itself. It is slightly misleading, however, to take the idea this far. Bartlett clearly realised he was way beyond his data and into the realm of speculation. He meant these ideas merely as a "suggestion" and put them forward "with some hesitation". The main function of the concept was, in truth, to explain his experimental data.

"What precisely does the 'schema' do?" asks Bartlett (p 207);

> "Together with the immediately incoming impulse it renders a specific adaptive reaction possible - It is, therefore, producing an orientation of the organism towards whatever it is directed to at the moment. But that orientation must be dominated by the immediately preceding reaction or experiences. To break away from this the 'schema' must become not merely something that works the organism, but something with which the organism can work".

It will be seen from the above that the term schema is something of a universal explanation for Bartlett and that it is rather vaguely defined and yet widely utilised. Northway's (1940) paper reviews Bartlett's different uses of the term and proposes that much of its conceptual vagueness comes from the origins of the concept in the work of Ward, Head and Rivers. We examine these ideas below.

Northway describes four quite separate uses of the term. These uses are fairly clear in themselves she argues, but confusion arises because one definition is often exchanged for another without explanation.

1. Schemata are considered as <u>forces</u> determining recall.
2. Schemata are the <u>forms</u> or the organisation in which past experience is stored.
3. Schemata are the storehouses or templates in which <u>content</u> is retained.
4. Schemata are compared to the notion of "<u>apperceptive mass</u>" or "<u>organised setting</u>".

From our own discussion above it should be clear that Northway provides us with a useful categorisation of Bartlett's usages. There is a genuine confusion caused by Bartlett's wide, all encompassing, use of the term 'schema'. Northway goes on to suggest that this confusion arises through Bartlett attempting to attack several different, though related, problems with this limited conceptual armoury. He was lured into this mistake by the varying uses of the term by Head, Ward and Rivers all of whom influenced Bartlett's theorising.

The Influence of Head

The concept of schema could if required be traced back almost indefinitely. It is suggested in the work of Kant, Lotze, the Wurzburg School and the Gestaltists.(1) The most prominent modern source of the concept, for Bartlett, was however the work of Sir Henry Head. Appearing first in Head and Holmes' (1911) paper on cerebral lesions the concept becomes clearer in Head's (1920) Studies in Neurology and fully developed in Aphasia and Kindred Disorders of Speech in (1926). Head and Bartlett were close friends, each in their major work (Remembering and Aphasia) acknowledging their mutual debt. Zangwill (1972) goes so far as to say, "This relationship with Head appears to have given Bartlett the essential intellectual stimulus which he

(1) The Wurzburg work in particular is acknowledged by Piaget as a forerunner of his own ideas - which are not totally without resemblance to Bartlett's.

needed to construct a general theory of memory". (Zangwill (1977) reported that Head considered Bartlett to be the only person who really understood his concept of schema).

Head had noted that cerebral lesion sometimes produced loss of awareness of limb position without loss of awareness of movement. Thus Head evolved his idea of bodily schema which represented bodily positions and which could rapidly incorporate each change in position and thus offer a means of motor control. Head's definition was, "That combined standard against which all subsequent changes in posture are registered before they enter consciousness".

Bartlett refined this idea in three main ways (for a lengthy and detailed discussion of this point see Oldfield and Zangwill (1942-1943).

a) Bartlett disliked the "standard" or "store house" concept implicit in Head. He preferred to talk of schemata as "living constantly developing, affected by every bit of incoming sensational experience," claiming that "the store house notion is as far removed from this as it well could be".

b) Bartlett also disliked Head's vague talk of schemata "rising into consciousness". He emphasises that even in Head's application of schemata to bodily change that, constantly motor adjustments are made with no awareness whatever.

c) "It is at once too definite and too sketchy", but, he goes on, " it seems the best single descriptive word available it would probably be best to speak of active developing patterns. I think probably the term 'organised set' approximates most closely to the notion required".

However, although Bartlett voices these criticisms (in Remembering p 199 and onwards) he by no means sticks to them. Especially, as Northway points out in that he regressed "to its use as a somewhat static standard" and a repository for content.

The Influence of Ward

We have already seen that Bartlett owed some of his interest in memory to Ward. But Ward has a more specific contribution to make. For he was the importer to Britain of the Lotzean idea of the presentation continuum. This idea included within it the basic premise of the activity of memory. In Ward's view interest and attention were the guiding principles of mental activity. The continuity of this activity became the principle by which material became absorbed within the presentation continuum. Bartlett himself discusses the similarity of his approach with that of Stout and Ward (op. cit. p 306-307) emphasising his use

of the concepts of activity, interest and continuity and his disregard of the more traditional associationism. Northway writes of Ward;

> "The principles of Ward's psychology are implicit throughout the work of Bartlett.... Ward restates the Lotzean point of view "that movements of attention determine what wholes will be formed and these are based primarily on interest. Ward does not think of items of content being absorbed and then integrated by some superimposed process of association, but rather, he considers the activity of the individual through his interest to be in its very nature integrating and developing".

Or as Bartlett (op. cit. p 308) puts it, associationism may tell us "something about the characteristics of associated details, when they are associated, but it explains nothing whatever of the activity of the conditions by which they are brought together".

Thus Bartlett clearly endorsed Ward's general approach and was much taken with Ward's notion of the 'plasticity of the presentation continuum'. But Ward's notion was far more concerned with the force or plan (which guided activity and attention) than with the storage of content. Thus Bartlett's use of schema is not truly consistent with his own Wardian psychology from which the notion came.

The Influence of W.H.R. Rivers

We have already seen above (in Section B) how much Bartlett's interest in social aspects of remembering, and particularly in conventionalisation, owed to Rivers. In

the second part of Remembering Bartlett draws parallels between the way information is absorbed by an individual and the way information is absorbed by cultures. He writes (p 299)

> "It may be that social conventions, institutions and traditions formed by persistent group tendencies constitute "group schemata"; just as the individual images, ideas and trains of thought formed by persistent interests constitute 'individual schemata'."

In other words Bartlett uses the same word to describe apparently similar processes in groups and individuals. Thus 'schema' is asked to describe mental processes, small group and cultural pressures. This extension of the concept cannot but lead to unnecessary confusion claims Northway; with some justification.

Conclusions

We have seen above that the diverse sources of the concept of 'schema' and the attempt to apply it over a too varied area of study led to considerable conceptual confusion.

The concept, as Broadbent (1970(2)) puts it, "had no list of defining properties, but was simply a label for something whose operation was illustrated by experimental results.....like others of its breed schema expired unregretted among mutual misunderstanding". Zangwill (1972) writes that the theory of schema "never very plausible, is perhaps best forgotten".

These comments seem to be most misleading in the light of developments since Neisser's (1967) publication of Cognitive Psychology - a theme we pursue in later sections. However, it is true that although Bartlett's forays into the field of social psychology and anthropology were most illuminating the attempt to transport schema from the individual to the social was always doomed to failure. It is only as a useful concept to explain individual cognitive processes that the concept survives today.

D. Remembering II - Other Aspects of the Book

The book begins with a chapter on "Experiment in Psychology". The views put forward here are in general consistent with Bartlett's general views on psychology which are presented in Chapter 16. The book then presents Bartlett's experimental results and his new theory of remembering. The main section we have not yet dealt with is Part II of the book "Remembering as a Study in Social Psychology".

In this section Bartlett presents no new experiments; essentially what he does is to present a case, based on anthropological data for the similarity of the processes of culture contact and transmission and the handing down of legends and folk stories with those processes observed in his own studies. A persistent theme is that much speculation about the mental processes involved could and should now be subject to empirical investigation.

The theme of conventionalisation is again prominent in this section. This process he now feels confident enough to state can operate by

a) Assimilation - to existing cultural forms.

b) Simplification - the "dropping out" of strange elements.

c) Retention of "subsidiary" or redundant details.

d) A process of social construction - the symbol develops along with the group's own development.

He moves on, in Chapter 17, to contrast his own study of conventionalisation with the theory of the collective unconscious. Conventionalisation is a demonstrable phenomenon, he writes, the ramifications of which can be observed and verified. He had found nothing (p 280) which "would indicate that the social past which inevitably helps to shape a group's new acquisitions persists in any other way than in its institutions, its current traditions, and in its preferred persistent tendencies".

He goes on to equate the memory trace of individual psychology with the collective unconscious in group psychology;

> "Both appear to assume that psychological material - images, symbols, ideas, formulae - are somehow individually preserved and stored up for use, either in the central nervous system of the individual, or somewhere in a persistent psychical structure in the possession of a social group".

There is some evidence that Intelligence, Bartlett writes (p 291), may be hereditary. But of the instincts which the collective unconscious depended on there was no evidence at all. The idea was "extremely confused" in his opinion and "far more careful and controlled study is required before any definite statement can be made".

Such an attitude is of course characteristic of Bartlett, and as we shall see in the next chapter, it was an attitude typical of his orientation towards Freudian and Jungian theory in general.

Surprisingly enough only one experimental study of conventionalisation, aimed at reproducing historically observed changes in the laboratory, appears to have been performed. T.H.G. Ward in 1949 managed to reproduce, by serial reproduction, actual changes that had occurred to a Macedonian coin design between the fourth and first centuries B.C. (Bartlett himself thought that Ward might have been a little lucky).

Bartlett himself concluded that no new concepts (in addition to those used to describe individual remembering) were required to explain the ethnological data he presented. Part II of the book is thus full of interesting discussion and is perhaps most notable for its clear statement of the case for experimental methods to be applied to this area.

E. Bartlett's "Return to Remembering"

(All quotes in this section unless otherwise attributed come from Bartlett's unpublished article "Return to Remembering").

Before we move on to an assessment of Remembering light can be shed on our discussion by Bartlett's own (1968) "Return to Remembering". This article is especially interesting for Bartlett's statements of the purposes of his book - purposes which should be born in mind when we attempt to judge the work. A lengthy summary of the article is thus presented.

> "When Remembering was being planned and written the general psychological bent was definitely romantic. In England strong influences came to me from C.S. Myers, W.H.R. Rivers, William McDougall and Henry Head. All of these were doctors of medicine, interested in diagnosis, and they all preferred to treat human reactions as wholes rather than by detailed analysis. From Germany came the even more "romantic" influence of Freud and the experimental methods and views of the Gestalt Group. Inevitably my approach was influenced by all these, and in a more general way perhaps by the stress laid on activity by James Ward and G.F. Stout";

thus Bartlett begins his "Return to Remembering".

Bartlett goes on to relate that, since 1932, psychology has become more "classical" and thus the methods of physiology and physics have invaded the discipline. Thus, he writes;

> "it is not surprising if Remembering should sometimes now appear to be less concerned with itemised and numerical analysis than it could and should be".

Having made these general points Bartlett moved to define more exactly the aim of his book. "I must first make it clear then that the title ....was chosen very deliberately". He goes on to state that memory may well exist in the sense that all experience may be stored away somewhere in the central nervous system. But he writes;

> "I did not set out to pronounce on any of these views. Whatever else they imply they all involve the undoubted fact that remembering does take place and my aim was to try to find out as much as I could about its character and implications as an active process, and as it takes place in the ordinary course of daily life, as free as possible from any specially imposed conditions other than those of the natural environment".

Elaborating on the theme he continues;

> "I did not say, I think I did not imply that literal retrieval is impossible, but I did imply that it requires special constricting conditions".

Such conditions obviously occur in rote learning for exams, actors learning lines and so on - it would be"ludicrous" writes Bartlett to deny this;(1)

"Nothing," he continues "that I wrote was intended to deny the possibility of this, and if any of the statements in the theoretical parts of the book seem to imply such denial they must have been badly phrased".

Bartlett clearly intended his work to be something of a naturalistic study of remembering as it occurs in everyday life. This stance enables him to dismiss Gauld and Stephenson's (1967) criticism (see next section). It is quite possible he argues for subjects, "to repeat exactly

(1) It is clear however that Bartlett would endorse Neisser's caveat, that the <u>precise</u> repetition of any movement or speech is extremely difficult if not impossible. Bartlett's famous discussion of "making a stroke in quick game such as tennis or cricket" (<u>Remembering</u> p 201-202) would seem to bear this out; "When I make the stroke I do not, as a matter of fact, produce something absolutely new and I never merely repeat something old".

the original material, or to identify their own errors" given the right "predisposing orders". But that, he points out, is simply not what happens in everyday life. "In the great majority of instances," he writes, "the past is being used to help interpret the present and is for that purpose reconstructed".

Bartlett supports this argument by pointing out 'good' remembering usually refers to the functional use of the past rather than parrot-like repetition of past events.

He then turns to the contentious issue of "turning round on one's own schemata". This phrase he writes was "never intended to explain anything". It was used as, "a descriptive phrase for something that actually does happen". Schemata can be used, he writes, in two different ways. Motor skills, "adaptive behaviour" in animals and much of perception is determined by schemata. On the other hand humans, perhaps uniquely, can use their schemata for recall and for thinking. It is this directed use of schemata that he refers to as a "turning round" process. The phrase was hoped to discriminate between a case where; "an organised past is operating immediately and directly to determine a current reaction" and where a subject used past experience to decide say between two interpretations of an ambiguous figure. The turning process, he writes, involves the observed phenomena of "analysis, condensation, rationalisation, temporal

re-arrangements, constructive imagination etc. to serve current interests".

Next Bartlett addresses himself to the problem of the "two store hypothesis". Much experimental work, he argues, (just as Craik and Lockhart (1972) argued) tends to predispose its own results and especially so in the field of memory. He goes on to criticise the concepts of short term and long term memory. He argues that the distinction does not hold, simply because, as his own experiments showed long ago, short term recall can be remarkably constructive and inferential while long term recall can also be merely duplicative. Thus the barriers between short term memory and long term memory would seem to be rather blurred and a better distinction, he writes, would be between duplicative and reconstructive processes rather than different stores. His argument hinges (just as Craik and Lockhart's does) on the overriding influence of the demand characteristics of a given memory task.

Bartlett pursues this line in the last section of his paper "Remembering Discrete Items". Here he criticises modern workers who, following Ebbinghaus, have attempted to divorce their stimuli of all meaning by using letters, digits, nonsense syllables inter alia. He writes;

> "Apparently there is little recognition that such methods inevitably predispose special ways of treating their results. An item reappears or it does not".

Such approaches have not only led Bartlett to despair. Neisser (1976) shares Bartlett's concern as we shall see below.

But we must leave the last word here to Bartlett himself;

> "The use merely of highly itemised material or of simple repetitive performance leaves out much that may be of great importance. They can indeed be valuable, and within their limits can yield valid results, but they cannot successfully be used by themselves to settle the full characteristics of remembering".

The main issues raised here by Bartlett; the purpose of his work; the implications of his theory to the two-store hypothesis and the competing claims of nonsense-syllable and realistic experiments are discussed in the following sections.

The issue of "turning round on the schemata" can be dealt with here, however. For this is surely another case of Bartlett trying to use the term schema far too intensively - as we have argued above. It is also a case of Bartlett's use of ambiguous language catching up on him - but we return to this issue later.

F. Remembering and Modern Studies of Memory

There is a stark contrast between Bartlett's naturalistic method and much modern strictly controlled, laboratory-bound, nonsense syllable experimentation. Despite this, and despite the fact that Bartlett had aims that were different in direction and scope to much contemporary work, Remembering does have several points of contact with modern memory theory. Some of these points are discussed below.

The Constructiveness of Recall

As we have seen, Bartlett treated recall as a constructive process - a stance which led to his influential repudiation of the 'trace' theory of memory. However Zangwill

(1972) reports three separate studies of recognition in which repeated reproduction appeared to have no effect on recognition of the original stimulus. This occurred even when the original had to be selected from among the subjects' own reproductions. This seems to produce strong evidence for the retention of some original trace. Gauld and Stephenson (1967) in a similar study, discovered that strict instructions to cut down errors in repeated reproduction not only achieved this aim but also enabled subjects to identify their own errors very clearly.

Zangwill (op. cit.) also reports experiments involving the immediate oral reproduction of aurally presented prose passages. This paradigm produced errors "almost-all" of which were errors of omission. Thus recall, claims Zangwill should be seen as <u>abstractive</u> rather than <u>constructive</u>. Kay (1955) also supported these claims.

Bartlett was relatively unmoved by such criticism. That such parrot-like behaviour can be induced, by strict conditions or, by overloading memory capacity or (more often) by the use of nonsense syllables (which are either recalled or not) Bartlett (1968) was only too ready to admit.

A modern Bartlettian might riposte to these criticisms by saying that recognition of the original stimulus, in such experiments, is easily explained by the reintroduction of new "cueing" information. Such a worker, Neisser (1967), has of course proposed a process of "analysis by synthesis" for recognition in both visual and auditory modalities. This view holds that perception

and recall depend on a constructive process which revives post-experience on the basis of critical cues.

As Zangwill himself (1972) points out, the evidence that "unwitting transformation in recall" does occur, espcially over long periods, "can scarcely be contested". And yet equally rote recall can and does occur.

The war then between "trace theorists" and "constructivists" still rages. Bartlett's contribution to this debate was to remind psychologists, in the 1930's of the existence of the alternative "constructivist" approach. His own approach was severely hampered, with comparison to modern work by the absence of modern information theory and computing concepts. But as we shall see in Section G today's neo-Bartlettians have not been slow to utilise these new tools to pursue Bartlett's general approach.

The Two Store Hypothesis

As Zangwill (1972) has hinted, and as Bartlett (1968) admitted, Bartlett rather avoided the traditional question of Long Term and Short Term memory stores. However he did, as we have seen, accept the concept of rote recall and the idea of an immediate memory span. He merely regarded both these cases as rather special instances where especially constraining conditions produced unusually constrained results. This stance is remarkably similar to that of Craik and Lockhart (1972). Their work (which sparked off much new thinking into "working" memory and "processing" versus "storage" models) claimed that the two store model was purely an artefact. Acoustic interference occurred in STM

experiments because subjects had no need to code the stimulus material any further than such "surface" characteristics. Long term storage however required processing to "deeper" semantic levels - when semantic interference would occur. Thus they hope to explain the different coding machanisms commonly claimed as defining characteristics of LTM and STM stores.

It is probably too early to judge the impact of this work. However it probably is true to say that the STM - LTM dichotomy is no longer as hard and fast as it once was. It is also true to say that the modern cognitive psychologists' approaches, emphasising motivation, interest and experimental constraints as determinants of memory are markedly similar to Bartlett's own.

The Use of Imagery

As Kessel (1972) puts it:

> "Imagery has hardly been an overworked topic in psychology over the past few decades. In fact, in the twenty year period from 1940 Psychological Abstracts contains only five references to imagery".

However as Kessel reports, within the current "cognitive experiential Zeitgeist" interest in the topic has picked up. Most notably research in eidetic imagery has re-commenced, (e.g. Haber 1970; Haber and Haber 1964) and the study of the role of imagery in recall (e.g. Paivio 1970) has also re-emerged.

Kessel (1972) relates Bartlett's theoretical position regarding imagery, pointing out that he viewed imagery as an

integral part of both thinking and remembering. The use of imagery, according to Bartlett, was one way of dealing with "situations at a distance". (Remembering p. 25). Images were a sort of sign, employed in thinking, which selected pieces from schemata - increasing the possibility of variation in recall. As Kessel indicates (1972 p. 157) Bartlett's views, "are markedly similar to those of contemporary writers," in that;

1) He stressed the advantages of imagery (especially visual) in promoting vividness, flexibility and creativity.
2) He stressed its disadvantages in being rather idiosyncratic and closely allied to affect.

Thus Bartlett's viewpoint, he considers, pointed towards studies of; individual differences in imagery; the effect of imagery on recall and creative thinking; a challenge largely ignored until events of late.

In addition to the revival of a cognitive psychology with a place for imagery (see also Neisser 1976 and Holt 1964) Bartlett's observations received an unexpected boost from the unlikely direction of studies on commissurotomy. In their famous paper, of 1972, Levy,Trevarthen and Sperry, produced evidence to suggest that each hemisphere handled information in its own distinct way. As Zangwill writes (1972);

> "In general, the right hemisphere proceeds by global impression and direct matching; the left, by sequential analysis of key features. This difference parallels almost exactly that between the visualiser and vocaliser as described by Bartlett".

In normal subjects, of course, both hemispheres are involved in all acts of perception. But Zangwill, at least, regards this finding as potentially of the utmost importance in leading to further information about our processing of sensory input and in our evocation of images.

Neurological Studies

Bartlett never cared much for neurological studies of memory. He appeared to have no interest whatever in promoting neurological investigations of his own work. Others, however, have shed light on various aspects of his work and Zangwill (1972) has reviewed some of the evidence.

One problem for Bartlett's theory seems to be that brain damage can cause very specific defects in memory - with apparently no influence on perception, language, emotion or even personality. Such findings are difficult to account for in Bartlett's theory, which stresses the close interlinkage of these functions. This evidence is not conclusive, however, as it does not rule out such interlinkage in normal functioning.

Retrograde amnesia produces a further problem. Recovery of lost memory most often occurs purely chronologically. This finding suggests the primary importance of temporal organisation in storage or, as contemporary opinion seems to prefer, in retrieval mechanisms. The concept of consolidation over time is also shown to be important. For Bartlett chronology was merely one organising factor among many (such as interest, biological importance, attitude and social factors). This suggests that temporal factors should be somewhat upgraded in Bartlett's theory to account for these findings.

Zangwill concludes his discussion (1972) by reviewing the powerful evidence for the cerebral localisation of specific amnesic syndromes in both STM and LTM. This last evidence again is not crucial to Bartlett's ideas. But it does show the potential value of an area Bartlett persistently ignored.

G. The Importance of Remembering; An Assessment

Some General Points

Broadbent (1970 (2)) contrasts Remembering with other 1930's work.

> "In America the early forms of behaviourism were restricted to claims of elementary stimulus - response links; in Europe the Gestaltists thought of experience as governed by field forces in the brain. Neither party allowed for anything so complex as the mixed and hierarchical levels of processing which Bartlett was discussing; nor for the intimate links of social structure and of individual psychology".

"In modern terms", continues Broadbent, "what he emphasised was the selective and constructive character both of perception and of memory".

Zangwill (1970) calls the book "a landmark". He cites four reasons for this:-

1. It marked a clean break with the German tradition.
2. It demonstrated that experimental methods could easily be applied to the higher mental processes - without necessarily resorting to introspection.

3. A concern with realistic everyday situations need not, the book showed, mean a departure from scientific procedures.

4. It showed the importance of social factors.

Broadbent and Zangwill have neatly summarised the historical position of Remembering. The book stood very much alone. It attempted to steer a middle course between the behaviourist approach and that of the Gestaltists. In doing so, it managed, eventually to found a new tradition for the study of man's cognitive behaviour. It was the historical reasons alluded to in Chapter 1 of this thesis, which were largely responsible for the slow response to Bartlett's ideas. Baddeley describes how the revival eventually happened (1976);

> "Bartlett's theory could be criticised as being too vague and complex to be testable; it is probably fair to say that for 30 years following the publication of Remembering, relatively little theoretical development occurred along the lines he suggested. Nevertheless, Bartlett has continued to have an important influence on the study of human memory, not only through his own students, such as Broadbent, Brown, and Conrad, but also through such U.S. psychologists as G.A. Miller and Neisser. In the introduction to his stimulating book Cognitive Psychology, Neisser (1967, p. 10) describes his approach as "more closely related to that of Bartlett than to any other contemporary psychologist". With the current trend away from the study of isolated words and with the growth of interest in meaningful material such as prose and pictures, Bartlett's work is likely to become increasingly influential. To give just one example, Bartlett's work on the recall of pictures has been largely neglected, and yet his technique of cued recall, in which he would ask the subject for specific details (e.g., "What, if anything, was the sailor smoking?"), would allow a much finer analysis of visual memory than is typically obtained in many current studies.

> "Furthermore, the development of the electronic computer, with its enormous capacity and flexibility, has made Bartlett's theoretical ideas seem much more viable. As early as 1954, Oldfield pointed out the analogy between Bartlett's account of human memory, in which existing schemata are used to store new information, and that of a computer, which must store patterns of events that have basic common elements. In both cases it is more economical to store a new event on the basis of an existing pattern, together with any deviations from that pattern, than to use a completely new set of storage locations for each item. A similar view was suggested by Miller (1968) and is incorporated into recent models of semantic memory, such as those of Quillian (1968) and Rumelhart, Lindsay, and Norman (1972)."

The 1970's have seen a remarkable resurgence of Remembering. Neisser (1976) refers to a host of "new Bartlettians" who "all follow Bartlett's emphasis on meaning and understanding as crucial for memory". (He lists; Sachs 1967; 1974; Dooling and Lachman 1971; Bransford and Franks 1971; Bransford and Johnson 1973; Paris and Carter 1973; Jenkins 1974 et. al).

The Neisser of 1976 is in fact even more overtly Bartlettian than the Neisser of 1967. He begins his book with; "Cognition is the activity of knowing; the acquisition, organisation, and use of knowledge". In the following passage he even goes to the lengths of accusing Bartlett of being unrealistic!

> "The fact is that we have almost no systematic knowledge about memory as it occurs in the course of ordinary life. Almost all the phenomena that a contemporary theory must explain are highly artificial: recall of word lists or nonsense syllables, identification of photographs that were included in a long series inflicted on the subject earlier and so on. Bartlett recognised this problem many years ago, but his demands for specific recall of page long "stories" read on previous occasions

> were almost equally unrealistic. Contemporary neo-Bartlettians are again setting their subjects the task of remembering brief texts, but the ingenuity of their methods does not alter the fact that no ordinary person would do such a thing if he could help it".

Thus Bartlett's approach to memory is still alive and well, as is his emphasis on semantic and interest factors (in the work of Collins and Quillian; Craik and Lockhart respectively). But another idea of Bartlett's - that of schema - is also still with us. Neisser (1976) reminds us of this, writing that Marvin Minsky is the most notable of many researchers using the term in artificial intelligence and computer simulation of human functions. Meanwhile Canter uses 'schema' extensively in his Psychology of Place (1976). In the field of motor skills, Fitts, Adams and Martineux are using the term. In memory Kirk H. Smith, S. H. Evans and of course Neisser are among those using schema. Posner's personal contact with Bartlett also shows in his influential work. Nearly all these researchers pay their respects to Bartlett's influence.

Remembering then is far from dead, indeed if Baddeley and Neisser are right it has never been more alive.

Some Criticisms

Much can be made of Bartlett's failure to quantify or categorise his data. But it should be remembered that one of Bartlett's main purposes (Remembering p 9) was to provide a naturalistic and descriptive study of phenomena never before scrutinised by psychologists. His aim, as we have seen in Bartlett's own "Return to Remembering", was

certainly not to produce a definitive account of remembering. Rather it was to describe the sort of process that occurred for the benefit of future studies. As he put it himself, (in rather dated language);

> "an attempt is being made to deal with a field of research in which suspected relations must be made as definite as possible before it can become fruitful to collect and correlate masses of results".

Indeed Bartlett could have used content analysis for stories, developed scoring schemes for pictorial reproductions and so on. But the work would, arguably, have lost as much in readability and vividness as it gained in quantitative accuracy - a dubious gain given Bartlett's intentions.

Perhaps, in any case, such a lack of quantification was inevitable for as Baddeley has since put it (1976);

> "The study of memory continues to be torn between Ebbinghaus's insistence on simplification (with its attendant danger of trivialisation) and Bartlett's emphasis on the complexities of human memory (with its danger of intractability). As the ensuing chapters will show, the study of memory is repeatedly influenced by this tension, which is sometimes reflected in open conflict between theoretical positions, at other times in drifts in fashion from one approach to the other. Given the richness and complexity of human memory, such conflict is both inevitable and healthy; neither approach is uniquely correct, and an approach which is productive at one stage of conceptual and technical development may be sterile at another".

Nevertheless it is quite possible (indeed it is easy), to criticise Remembering on several counts.

For example;

a) The sample sizes were small and selective - ludicrously small for a general theory of memory.

b) The experimental methods were highly selective. They included nothing, for example, on recall over periods of less than fifteen minutes.

c) The main theoretical concept of 'schema' is ambiguous. For 'schema' has several different meanings (from several different sources), each meaning being appropriate for part of the data. Bartlett's theory becomes most confusing because of the apparent attempt to extend the many meanings over all the data.

d) Bartlett himself undertook all the experiments and interviewing of subjects. There was no objective scoring of responses. Hence there is a large possibility of bias, prompting and selectivity in the results.

e) The concept of schema was just plain inadequate to describe the complexity of the data. This becomes especially apparent regarding "turning round on the schemata".

f) Even the main experimental findings are only selectively and casually reported in everyday, ambiguous language.

g) The importance of temporal factors is underplayed.

h) The general theory goes far beyond that the data collected - attempting to account

for (inter alia) imagery, creative thinking and even consciousness itself.

Further if we use the insights given to us by Popper we would probably find that Bartlett's theory would have little predictive value and lack "verisimilitude". Rather like Freud's psychoanalytic theory however hypothetico-deductive concepts are readily drawn from Bartlett's work which can produce testable research hypotheses. Indeed we have reviewed above many such studies of hypotheses from Bartlett's work.

But criticism slating Bartlett's lack of experimental rigour and use of non-operational or at least non-public language are in danger of missing the point. Bartlett (completely unlike Freud) welcomed challenge and elaboration to his theory. He regarded it, justly in this author's opinion, as a tentative first step into an unexplored area; as a stimulus to further research; as a descriptive study of every day events. Throughout Remembering Bartlett advises his reader to treat his findings not as definitive but as stimulating and descriptive.

But perhaps (as is the case with Freud) the most remarkable thing of all is that given; his "shocking" lack of experimental rigour; his "negligent" lack of objective scoring; his "unfortunate" lack of suitable theoretical equipment and his use of ambiguous language; Bartlett did succeed in formulating a theory which was hugely influential, stimulated much research and is still very much with us today.

CHAPTER 16

BARTLETT'S GENERAL INFLUENCE

> Bartlett had; "No theory, no statistics, no methodology!" L. S. Hearnshaw (1964 p. 217)

A. Introduction

It is probably true to say that Bartlett's only dogmatic stance on the nature of psychology was that psychologists should never be dogmatic. Zangwill (1970) relates that Bartlett, "had what can only be called a temperamental distaste for anything smacking of doctrine and thoroughly disliked cults and sects of any kind".

Thus it is hardly surprising to discover that Bartlett never formalised his own ideas on the nature of psychology. Nevertheless he did write a large number of critiques of various schools of psychology and a number of articles on particular methodological problems within the discipline. These articles, combined with the recurrent features of Bartlett's own work, provide the basis for the formulation of "Bartlettian psychology" which follows.

It could be argued that the exercise of formulating a Bartlettian psychology is a rather unnecessary one. Certainly Bartlett himself never felt inclined to do so. However, as we have seen in Chapter 13, Bartlett was a very influential teacher and this chapter attempts to encapsulate the sort of psychology which Bartlett, implicitly at least, passed on to his pupils.

Broadbent has attempted to outline Bartlett's psychology;

> "There were a number of broad strategies of scientific method which his students learned from him. First and foremost empiricism: a respect for concrete observation and a casual disregard for arid formalization. Next, lack of personal involvement in ideas; and the importance of weighing other people's according to the value of the idea itself and not the status of the source. Thirdly, flexibility and the cheerful willingness to admit past errors. Lastly, constant awareness of the complexity of psychological mechanisms.
>
> These lessons were passed on by infection; and they were valuable ones".

Also Hearnshaw has written (1969);

> "Much more influential (than McDougall's work) is the essentially empirical, anti theoretical, Cambridge school, under its successive directors, C.S. Myers, F. C. Bartlett and O.L. Zangwill - experimental, but not methodologically doctrinaire, concerned with investigating manageable problems and keeping close to the complexities of human behaviour as found in real life situations, occupational, clinical and social". (my emphasis)

The following characterisation of Bartlett's psychology is essentially an attempt to expand those of Broadbent and Hearnshaw. It is intended as a guide to Bartlett's thought rather than a definitive record. I have attempted to outline it in terms of eleven distinctive features.

The Eleven Distinctive Features of Bartlettian Psychology

1. Schema

See Chapter 15.

2. "Effort After Meaning"

As we have seen in Chapter 15 this notion appears throughout Bartlett's cognitive studies.

Perhaps the most important implication of the idea is that meaning becomes crucial for recall and perception - a point not lost on Neisser's "neo Bartlettians". The gradual erosion of behaviourism in the 1960's and 1970's has been characterised by the rediscovery, on several fronts, of the importance of meaning. Broadbent's famous filter model of selective attention was elaborated by Treisman on the grounds that selection for consciousness depended on meaning to the individual. N. F. Dixon(1) and Neisser (1967) have also reached the conclusion that a pre-attentive search process selects material for further analysis on the basis of meaning. The process approach to memory of Craik and Lockhart and Collins and Quillian's semantic storage model also depend heavily on meaning. George Miller's advocacy of psycholinguistics evolved from the inability of behaviouristic approaches to language to explain the importance of meaning in linguistic processes.

Meaning then is today a problem that psychologists are not quite so scared of. The rush to avoid the problem, in cognitive studies, usually took the form of using stimuli as devoid as possible of all possible meaning i.e. nonsense material. Bartlett argued long ago(and Joynson 1970; 1972 revived the argument) that this simplification of stimuli did not by any means lead to simplification of response. The individual would always attempt to invest

(1) In Dixon's (1971) Subliminal Perception; The Nature of a Controversy.

material with meaning. Furthermore, he argued, this attempt would affect the handling of information. Bartlett summed up this "behaviourist's fallacy" in 1936;

> "Constancy of objective determination is obviously consistent with variety of subjective attitude. Equally, since the human organism has grown up in a very varying environment, variation of objective circumstances is consistent with constancy of subjective orientation. It often becomes a matter of nice consideration whether the objective conditions or the orientation of the organism are the predominant determinants of the response. Neither can be neglected but when the latter takes the lead it is folly.... ..... to stick to the ideal of constant objective conditions merely because this is formulated and accepted by other sciences. (My emphasis).

3. The Cognitive Approach

It was not without reason that Neisser (1967) wrote that his own approach was, "more closely related to that of Bartlett than to any other contemporary psychologist". Bartlett's approach is markedly similar to modern cognitive studies in three ways;

a) Bartlett stressed the close interrelationships of memory, perception, imagery, thinking and even personality, interests and motivation;

> "None can set a ring round Memory and explain it from within itself.... (it) is not a completely independent function, entirely distinct from perceiving imagery or even from constructive thinking".

b) Bartlett also demonstrated that the higher mental processes were examinable by psychologists. As Holt (1964) and Kessel (1972) and many others have pointed out the higher

cognitive functions were scandalously ignored until the 1960's. As Neisser (1976) puts it;

"From the First World War to the 1960's, behaviourism and psychoanalysis (or their offshoots) so dominated American psychology that cognitive processes were almost entirely ignored. Perception, the most fundamental cognitive act, was studied primarily by a small group following the "Gestalt" tradition and a few other psychologists who worked on the measurement and physiology of sensory processes. Piaget and his collaborators studied cognitive development, but their contributions received little recognition. There was no work on attention. Research on memory was never entirely abandoned, but it dealt primarily with the learning of "nonsense syllables," in tightly defined laboratory procedures of little generality. As a result, the public image of psychology was that it dealt chiefly with sex, adjustment, and behavioural control.

This situation has changed radically in the last few years. Mental processes have again become a lively focus of interest. A new field called cognitive psychology has come into being. It studies perception, memory, attention, pattern recognition, problem solving, the psychology of language, cognitive development, and a host of other problems that had lain dormant for half a century. Technical journals once top heavy with articles on animal behaviour are now filled with reports of cognitive experiments, and new journals are mushrooming: Cognitive Psychology, Cognition, Memory and Cognition, Perception and Psychophysics. Grants for cognitive research are easily obtained, and nearly every major university has a cognitive laboratory. Piaget's work has been rediscovered and hailed.

There were several reasons for this turn of events, but the most important was probably the advent of the computer. This was not just because computers allow one to conduct experiments more easily or analyze data more thoroughly, though they do. Rather, it was

> because the activities of the computer itself seemed in some ways akin to cognitive processes. Computers accept information, manipulate symbols, store items in "memory" and retrieve them again, classify inputs, recognize pattens, and so on. Whether they do these things just like people was less important than that they do them at all. The coming of the computer provided a much-needed reassurance that cognitive processes were real; that they could be studied and perhaps understood".

(Things were not quite the same in Britain - as Neisser omits to point out - for here, as we have seen Bartlett's pupils (notably Broadbent and Mackworth) were studying attention and memory rather ahead of the revival in the United States.)

c) Bartlett's approach was also close to that of modern cognitive psychologists in the sense that he was interested in the form of internal representation. His approach was the natural forerunner of modern 'black box' approaches and predated the current interest in internal coding.

4. Social Factors

Bartlett was always keen to point out the existence of possible social influences on cognitive processes. His interest in social psychology was however more methodological than substantive.

However the anthropological influence, especially in his early work is considerable and his early studies of the

transmission of folk stories, conventionalisation, and culture contact were formative to Remembering.

5. Interest in Applied Psychology

Second to Remembering Bartlett's interest in, and promotion of, applied psychology are perhaps his greatest legacy to British psychology. Psychology and the Soldier and The Problem of Noise were useful contributions but more important were Bartlett's committee activities (see Chapter 13). As Broadbent 1970 (1) says "If any man is to take credit" for psychologists' help for the space programme, communications systems and electric cookers, "that man is Sir Frederic Bartlett". He is referring, of course not so much to Bartlett's own concrete achievements but more to the whole tenor of Cambridge psychology which Bartlett did so much to influence.

6. An Active Image of Man

Throughout his career Bartlett was concerned to keep what he called "the real man" at the centre of his studies. Realism, at least as Bartlett saw it, was more important than having a well founded philosphical account of the nature of man. He was constantly scathing of behaviouristic "puppet" experiments;

> "The man became a puppet, activated by strings and every time a particular string, or any combination of strings is pulled, exactly the same result follows. But animals are not like that because as soon as any behavioural

> response is set up it has to be continued not only on a basis of the outside stimuli, or the strings but on that of the further clues which come from inside the responses themselves". (1955 p. 209).

Ward's notion of an "active, striving, valuing" individual was fast becoming extinct, during the hey day of behaviourism, in psychological laboratories all over the world. However, a Wardian-inspired 'Bartlettian Man' was alive and well in the laboratory at Cambridge. He too was an active, free creature who could not help but bring along his own disposition, interests, past history and genetics to the experiment. (He might even be in a good or bad mood!) Eysenck was to repeat these criticisms of much of experimental psychology some forty years later (Joynson 1972 p. 40). In other words, for Bartlett, people were essentially different from things. Psychological functions are unique in the known universe, he points out, because they appear to vary according to the psychological material with which they deal. That is, according to a person's distinctly individual reaction to a stimulus. This is of course exactly Joynson's point when he says that inner conditions need have very little to do with outer, physically observable ones.

Such considerations led Bartlett on to his famous pronouncement that "the psychologist should not stand in awe of the stimulus". This was for the obvious reason that a simple S does not lead to a simple R. This theme runs through Remembering and it is the emphasis on this commonsense point, that people are different, and make individual responses which makes the book such an important advance on the work of Ebbinghaus.

In summary then, common sense convinces Bartlett that the methods of physical science cannot be imported wholesale into psychology. "The ideal of simplifying the R by cutting it off from others with which it is normally combined, is all very well in its way but it is a dangerous and slippery ideal in Psychology."

As Miller Gal anter and Pribram (1960) said, and as Joynson might have said "living organisms are complicated, devious and poorly designed for research purposes and so on". The U.S.A. behaviourists had departed from this common sense caveat and much disappointment was to catch up with them. Bartlett admitted it from the outset.... but it is far from being a pessimistic approach, as Miller et al suggest. What could be more depressing and intuitively more lunatic than the Behaviourist's "puppet on a string?"

7. Non Dogmatic, Piecemeal Approach

Bartlett (1936 and 1969) wrote that it was his belief that most of the valuable work in psychology had come from work on practical problems. He said that this was because investigators were then able to ignore theory and just work on a clearly defined problem. In the same piece he says that "Complete systems and schemes of psychological explanation are the biggest stumbling block to the progress of psychology".

Cambridge psychology was designed to be flexible and adaptable. It was deliberately not allied to any philosophical background, apart possibly from a grasp of basic

scientific procedure. After all, philosophies change and can always be wrong. Early behaviourists, wrote Bartlett (1955), spent a few years carefully collecting facts and then built up "on these facts an enormous superstructure of systematic belief, alleged to cover the whole range of animal and human life and experience, one of the main merits of which will be that it differs from every other such superstructure raised by every other investigator".

Of early German psychology, Bartlett was equally scathing. He clearly regarded their schemes as grandiose - but sadly misguided and leading to stagnation, "Partly they worked their ideas to death," he wrote in 1955, "partly they were interrupted by the War". He seemed to view this early "Sensationism" more as a "pretentious great monument" than a serious attempt at science. "They wrote a lot of prodigiously long and often very boring reports in highly technical German".

It is interesting to compare his views towards the early Germans to his views on the later behaviourists. Essentially he puts forward the same arguments ..... "What they did helped to fasten upon scientific psychology an urge for the all embracing theory, which it has taken fifty particularly strenuous years partially to overcome". He had a life-long penchant against any form of grand theory in psychology. He wrote sadly of the Germans' flight from the laboratory as they grew older.

"Perhaps when anybody grows oldish, especially if he is a psychologist, and more especially if he is a German psychologist, he turns readily towards systems and theories .... and away from the slogging of the laboratory".

As we have seen in Chapters 1 and 2 there are few today who regard a unified psychological "grand theory" as desirable let alone possible. Bartlett again was in tune with contemporary psychology.

8. Dislike of Tests and Statistics

Bartlett's own writings are almost completely free of anything even vaguely statistical. In 1936 he wrote of psychology the "Elaborate theories of statistics I would leave out altogether".

Helpful though it may have been to Bartlett to concentrate on the process of memory in Remembering, Broadbent (1970(2)) points out two difficulties. Firstly it put a huge strain in Bartlett's intuition and secondly it made his conclasions rather less public and hence more ambiguous than might have been the case.

What is certain however is, that partly as a result of Bartlett's foible, Cambridge psychology remained refreshingly free of jargon and mathematics.

On personality tests (from Heim 1970) Bartlett, in a fashion which suggests he was a fan of Oscar Wilde's, said "I don't know .... they may be all right. They always seem to me to overestimate the self knowledge of the subject and to underestimate his sense of humour". He

certainly never encouraged the development of this work at Cambridge .... except for practical wartime selection purposes.

9. Lack of Physiology

It is somewhat surprising, especially in a modern context, to realise that Bartlett, that well known world authority on memory, knew little and probably cared less about the physiology or neurology of memory. Certainly no mention of these occurs in his work (see also Chapter 15).

There seem to have been two reasons for this omission. The first was that Bartlett, as Myers had been before him, was acutely concerned to build up psychology as a distinct independent discipline. Pure psychological explanation of behaviour helped this cause. Secondly Bartlett's own theory of memory was not really detailed enough for physiology or neurology to shed much light on. As we have seen (Chapter 15) even current neurological evidence is largely equivocal regarding the theory of Remembering.

10. Admissibility of Introspection

Again in this area Bartlett presents views which have recently returned to enjoy some vogue (see Holt 1964, Kessel 1972, Joynson 1972). Bartlett (1936 and elsewhere) maintains that there is no reason whatever for not allowing the use of introspection. He points out that it is reliable and useful, as well as being indispensable in some areas such as the study of pain, imagery, dreams and so on. Indeed

he points out it is often the only means available for studying such phenomena, which are surely the province of psychology? Bartlett (1936) says "There is no need that we should artificially restrict ourselves to the study of such conditions of response as can be observed by a second or third person. Very often the most valuable information can be given in terms possible only to the person himself who responds. Always such information should be sought and sometimes it may even be the only sort of information possible".

This stance is of course the logical result of Bartlett's eclectic non dogmatic approach - why should we not study introspection? Surely it can be used (or mis-used) just as any other means of investigation?

11. Dislike of Behaviourism

There is only one direct attack on Behaviourism by Bartlett apparently in print. This is a review of Watson's Behaviourism in Mind 1927. However the rest of his writings are liberally scattered with sideswipes at the school.

Bartlett's main objection is that behaviourism was a good idea which was carried much too far and became a dogma. Thus of Behaviourism he says, "It signalled a great fall. The last remnants of caution have disappeared". He was full of praise for Watson's previous work (Watson 1914;1919) regarding them as stimulating books especially in pointing out possible applications of animal studies to man.

In 1919 however Watson, for Bartlett, becomes a "wild theoretician" and makes "enormous and unwarranted use" of the conditioned reflex. Other more specific criticisms of Behaviourism have been made by Bartlett (see point 5 here). The main one in this piece is the danger as he saw it, of making extravagent claims.

Bartlett's anathema is not, however, restricted to purely Watsonian behaviourism. In 1955 he talks of many U.S.A. researchers. "They also ran, they even galloped towards the all embracing theory". He echoes Koch's often made point that much time was wasted in trivial experimentation in the search for this "great theory". In 1955 he also says of his recent trip to America that in many ways it was just like going back twenty years in time - except for vastly more sophisticated techniques and instrumentation.

> "There is still much experimentation on animal learning and behaviour of a kind which, apart from greatly improved instrumentation and a tremendous lot of talk about motivation - not in my opinion very effective talk - might almost as well have been planned in the early 1900's".

C. <u>The Implications of Bartlett's Approach</u>

Having thus characterised Bartlettian psychology we can begin to consider the importance of his position. It was certainly a modern one in recognising the limitations of behaviourism <u>and</u> of subjectivism whilst emphasising careful experiment and observation. It becomes even more creditable when one considers the activities of Bartlett's contemporaries in Germany and the U.S.A.

Bartlett (1955) looked back on the early part of the century and the work of Freud and Pavlov. Characteristically he recognises the merit of their work - but clearly does not regard them in any way as scientific psychologists.

Of Freud he says;

> "All of Freud's enthusiastic adherents among the experimental psychologists regarded him as preeminently a great scientist. In fact he never, well hardly ever, carried out an experiment in the strict sense of the word, and he made no pretence to use what is often considered to be the indispensable method of the experimental scientist, that of quantitative measurement. His method was the case study; his strength the most complete loyalty to facts, as he observed them, an unshakable belief in his interpretation of the facts a vast power of intuition or insight, and a great sweep of brooding imagination. In fact he raises in a most intriguing form the question of criteria by which a man is to be called a scientist. It may be that all the great scientists are people who work as artists in a field which everybody considers to be scientific.
>
> Whatever may be the ultimate verdict on the sombre picture Freud painted of human life and thinking, there is no possible doubt that his impact on experimental psychology was terrific, or that its chief effect was to make the whole subject more humane."

Later in the same piece he writes of Pavlov's discovery of the conditioned reflex.

> "Another formative force now appeared upon the scene. I.P. Pavlov had, by experiment, discovered the principle of the 'conditioned reflex' in the late 1890's. He described and developed his work in a series of brilliant publications from 1903 to 1928. It seemed that here was a genuinely objective method by which the learning processes of animals and men,.... could be studied and understood".

Thus while appreciating the insights provided by both these hugely disparate workers, he clearly believed, in a very common sense fashion, that psychology could only be made scientific by a compromise of the two.

> "Speaking only in a metaphorical sense, it appeared that Freud, the extreme subjectivist, and Pavlov, the extreme objectivist, could join hands in putting the real man in the centre of psychological study, though when their views were rushed to extremes, as they often were, the two could only fall apart and glare at each other".

Bartlett, during the 1920's and 1930's guided Cambridge psychology between the mysticism of Freud and the sterility of Watson, Hull and indeed Ebbinghaus. He also avoided Gestaltism in his drive towards an atheoretical, eclectic and concrete psychology. He managed this with great success. In doing so, as we have seen above, he managed to anticipate many of the central ideas of today's cognitive psychology.

It is Broadbent's belief (Broadbent 1975) that U.K. psychology positively benefitted by missing out on its radical Watsonian phase. Bartlett was by no means responsible for this alone. But by maintaining close contact with practicality and common sense at Cambridge he clearly played an important part. Hearnshaw (1964) goes along with this idea. He allots to Bartlett a "Very dominant role in British psychology from 1920 onwards", both in influencing the subject matter and approach.

D. Last Words

On the negative side, since Bartlett's departure the Cambridge laboratory has developed some of the areas

which he persistently ignored. Animal studies, physiological psychology and psychological testing have all gained in strength. But I intend to dedicate the last words here to the positive impact of Bartlett's work. Possibly Bartlett's most eminent pupil, Broadbent, ended his obituary of Bartlett thus;

> "On a longer time-scale, however, his ideas are likely to survive and even to become more important as the years go by. The development of mechanical systems for processing information has now, as he was one of the first to recognise, provided the theoretical language which his factual observations always needed. In the models which modern investigators construct one finds selective operations upon the input, storage of state rather than transition information, enormous emphasis upon probability as affecting the optimum encoding for memory, analysis of false perceptions and memories as an index of synthetic operations by the man himself, and so on. The mathematical sophistication of these authors might have produced a wry expression on Bartlett's face; but the concepts with which they are operating are his.
>
> There is little possibility now of such concepts vanishing again from the subject; and it is fascinating to note that there are stirrings again of interest in social pressure upon the individual, and the ways in which language and thought are moulded by society. I myself feel that some of Bartlett's insights have still not had their full impact, and they will come into their own in the next generation. But even if this is not so, those ideas which are already fully appreciated have a secure place, and are unlikely to be seriously challenged".

PART 5; POSTSCRIPT

CHAPTER 17

DEVELOPMENTS SINCE 1939

A. Sir Frederic Bartlett; The Years 1939 - 1969

Bartlett became heavily involved in the psychologists' war effort. He was enrolled as a member of; Medical Research Council; Investigation Committee on Brain Injuries; Flying Personnel Research Committee (R.A.F.); Military Personnel Research Committee. The Cambridge Laboratory became the centre of much wartime research (see B below).

One of the outstanding pieces of research was conducted by a protege of Bartlett's, Kenneth Craik. He rescued a cockpit from a crashed fighter, fitted it out as a simulator and proceeded to study flying skills under various conditions. At the same time Craik wrote his famous monograph The Nature of Explanation. This book proposed the explanation of human behaviour in terms of the cybernetic machines then newly available. This idea, together with the'information theory' proposed after the War by Norbert Wiener (a good friend of Bartlett) and Shannon set the tone for much of the information processing research in the psychology of (especially) the 1950's.

The War only served to heighten Bartlett's already large interest in applied psychology. The M.R.C. became even more firmly entrenched in the laboratory. Bartlett and the M.R.C. Secretary, Sir Edward Mellanby were largely behind moves to set up an Applied Psychology Unit at Cambridge in 1944. Craik was director designate when he was tragically killed in a road accident. Bartlett's affectionate obituary shows both his feelings and admiration for Craik.

Bartlett himself remained Director of the new A.P.U. until 1953. In that year the A.P.U. was finally separated from the laboratory and took up its current residence in Chaucer Road. In 1952 Bartlett resigned his Professorship which post O.L. Zangwill has held ever since. The A.P.U. has become a leading unit in its field. The first applications of Craik's ideas came in Mackworth's studies of vigilance and in Broadbent's studies of attention. Arguably Bartlett's and Craik's early advocacy of the information processing paradigm enabled British psychology to lead the world in the area of attention and memory throughout the 1950's. Broadbent (1975) concurs with this view, and adds that Bartlett's approach made the assimilation of the radical behaviourism of the 1950's very easy in Great Britain.

Also after the War Bartlett sponsored the setting up of the Nuffield Ageing Research Unit, under A.T. Welford near Cambridge. In 1950 he was elected President of the B.P.S. In 1958 Bartlett published his last book Thinking: An Experimental and Social Study. It marked the culmination of many years of work on the topic. Bartlett saw thinking as in some ways a mental parallel to bodily skill. In the book some ideas from Remembering are elaborated. His original 'schema' interpretation of skill had been grossly elaborated by information theory during the War and the final statement of this appears in Thinking.

For the rest of his life Bartlett remained active on committees concerned with the M.R.C., N.I.I.P. and his own A.P.U. He died on September 30th 1969 three

weeks before his 83rd birthday. Broadbent, one of the foremost of Bartlett's pupils wrote;

> In those later years also he continued his active committee life remained an active consultant of the Applied Psychology Unit, and indeed up to the time of his death was still to be seen at meetings. To the last his comments were valid, kind, important, and never those to be got from anybody else".

B. The Laboratory in World War II

(Most of what follows is taken from Vernon and Parry's excellent 1949 account of Personal Selection in the British Forces).

Kenneth Craik, as we have seen, devised his famous Cambridge cockpit to investigate flying skills, especially in relation to fatigue and drugs. Craik also investigated control and aiming of tank guns. Bartlett's papers from the War include papers for the R.A.F. on; selection and personality of air crews; Morse training; auditory tests; selection of wireless operators; effects of benzedrine; instrument controls and display.

Early in the War the laboratory was asked to assist in the improvement of selection procedures, mainly for the R.A.F. Psychometric tests were devised for 'g' and verbal intelligence. Selectors were trained at Cambridge to use standardised interviews in conjunction with suitable tests. Vernon and Parry conclude that these procedures probably had little effect on wastage but did make the R.A.F. "test conscious". A notable success was however Drew's work

on an aptitude test for morse operators. Alice Heim also devised her successful AH4 test for the selection of A.S.D.I.C. operators.

Other efforts such as "Craik's predictor" (for gunnery skills) and aptitude tests for various A.T.S. employments fell by the wayside. Craik however was important again in pointing out the need for the re-design of equipment which could often ease selection problems.

But undoubtedly the main contributions of the Cambridge laboratory were in the areas of motor skill and vigilance to which we have referred above. Bartlett (1941) for example made a significant break with previous studies. Prolonged performance in the simulator brought about two main results.

1) The perceptual organisation of the input from the instrument display, built up during training, gradually broke down. Attention would be narrowed to single instruments and thus peripheral cues would be missed.

2) Self imposed or "acceptable' error limits became more lax with increased fatigue. Pilots failed to notice deteriorating performance of one part of a complex operation - which often led to a snowballing effect as this one component could throw out the whole organisation of the skill.

Broadbent summed up this work, which started a whole new field in the study of skill thus;

> "In his (Bartlett's) laboratory strange fresh ideas such as information theory were being applied to human performance; and above all he was kindling enthusiasm.
>
> The views he was putting forward have been published only in various separate articles, and probably never formed a complete system. Indeed that was their strength, because they were a flexible method rather than a rigid dogma. The key concept was that of skill: the ability of men to produce for each new situation a fresh and yet perfectly adapted sequence of movements. No prewar model comparing the brain to a telephone switchboard could cope with such facts. Rather they required a subtle and hierarchically organised system which could predict the future, launch actions at appropriate times, handle local difficulties by peripheral closed-loop subsystems, remember for brief periods the stage reached in a continuous process, monitor its own level of performance and adjust to inadequacies, and so on. When such a system is stressed, it would yield first by errors of timing or of integration between sub-units of the performance, rather than by crude forms of breakdown. Each of these topics formed the start of a whole line of research, on which were engaged the bright young men who now in their turn are filling the chairs of psychology".

C. A Hundred Years of the Cambridge School 1877 - 1977

At the time of writing it is just over one century since James Ward made his first application for psychophysical apparatus. For the first third of that century Cambridge psychology progressed with painful slowness. When progress <u>was</u> made it was largely due to the personal efforts of Foster, Sidgwick, Ward, Rivers and Myers. Considering the relatively rapid progress of psychology in

America and Germany the administration of Cambridge University comes rather badly out of the story of Parts 1 and 2 of this thesis.

Our four main representatives of the Cambridge School, however, fare rather better. Ward, Rivers, Myers and Bartlett were all men with imagination and intellectual conviction. Perhaps most importantly all four men saw the importance of taking practical and political measures to see psychology become established. Without this facet of their characters their contributions would have been far, far smaller.

All four men also produced important intellectual work, while Bartlett produced one of British psychology's very few classics. It is easy to criticise much of their work from our contemporary standpoint. Thus in this thesis I have attempted to set their work as far as possible in its context (hopefully avoiding the twin perils of over criticism through hindsight and ancestor worship).

This thesis has been deliberately selective leaving out many interesting developments both inside and outside Cambridge. This device has, hopefully, the compensations of highlighting the internal historical development of the Cambridge School and the influences of each man upon the other. If these two aims have been achieved then this thesis has served its main purpose.

But I would like to end this work with a last thought about C.S. Myers. He, of our four psychologists, made a quite remarkable <u>administrative</u> contribution to the growth of our discipline.

In how many other young sciences can a single man have organised, and almost entirely paid for, one of the world's best laboratories; and then gone on to found a unique institution for the application of the same discipline? His perhaps is the most remarkable story of all in this history of the Cambridge School.

APPENDICES

1. Photographs of the Cambridge Laboratory at Mill Lane.

2. Notes on Archival Sources.

3. A "Time Line" of Cambridge Psychology 1860 - 1939.

4. Select Bibliographies and Biographical Material for Bartlett, Myers, Rivers and Ward.

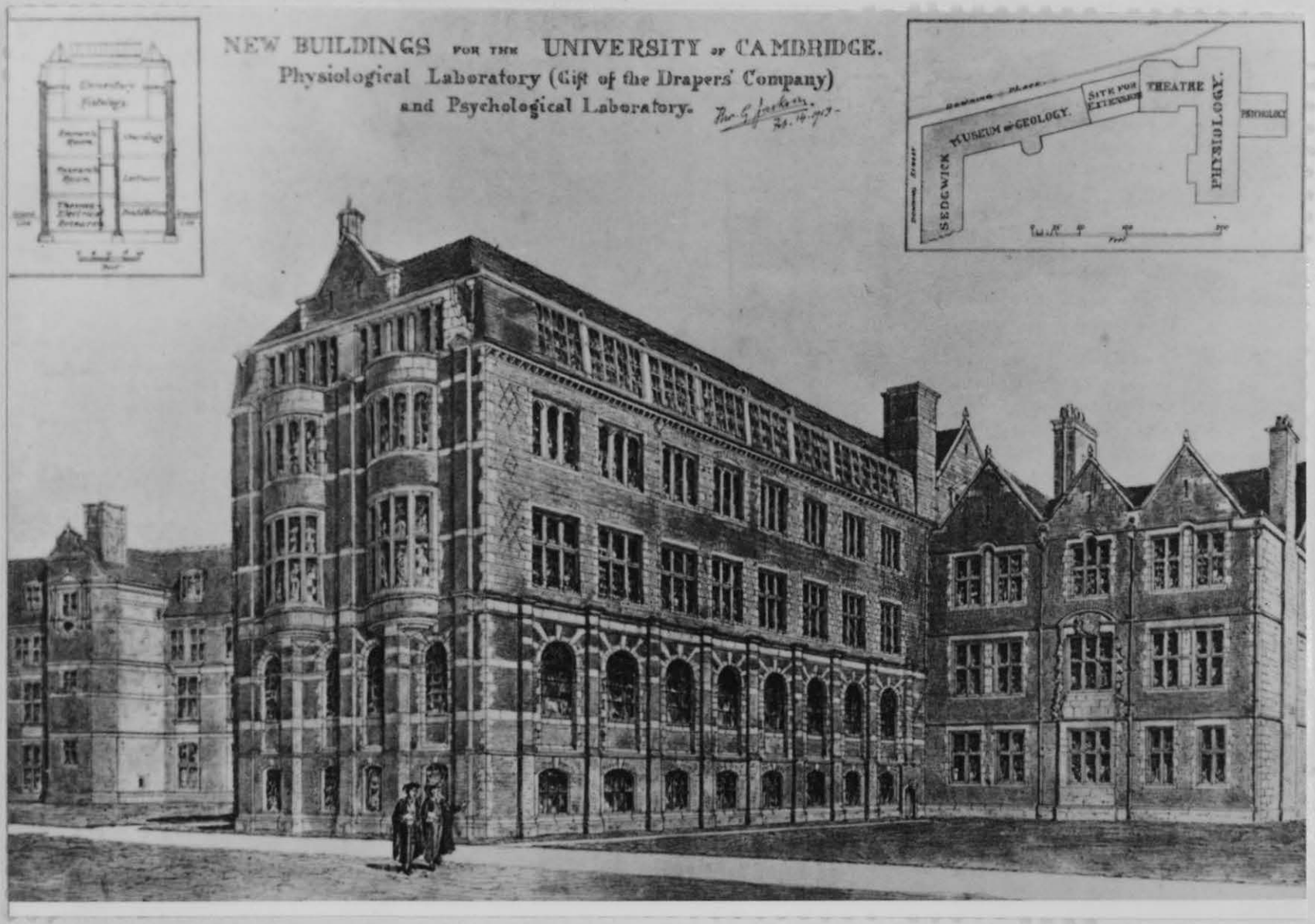
APPENDIX 1

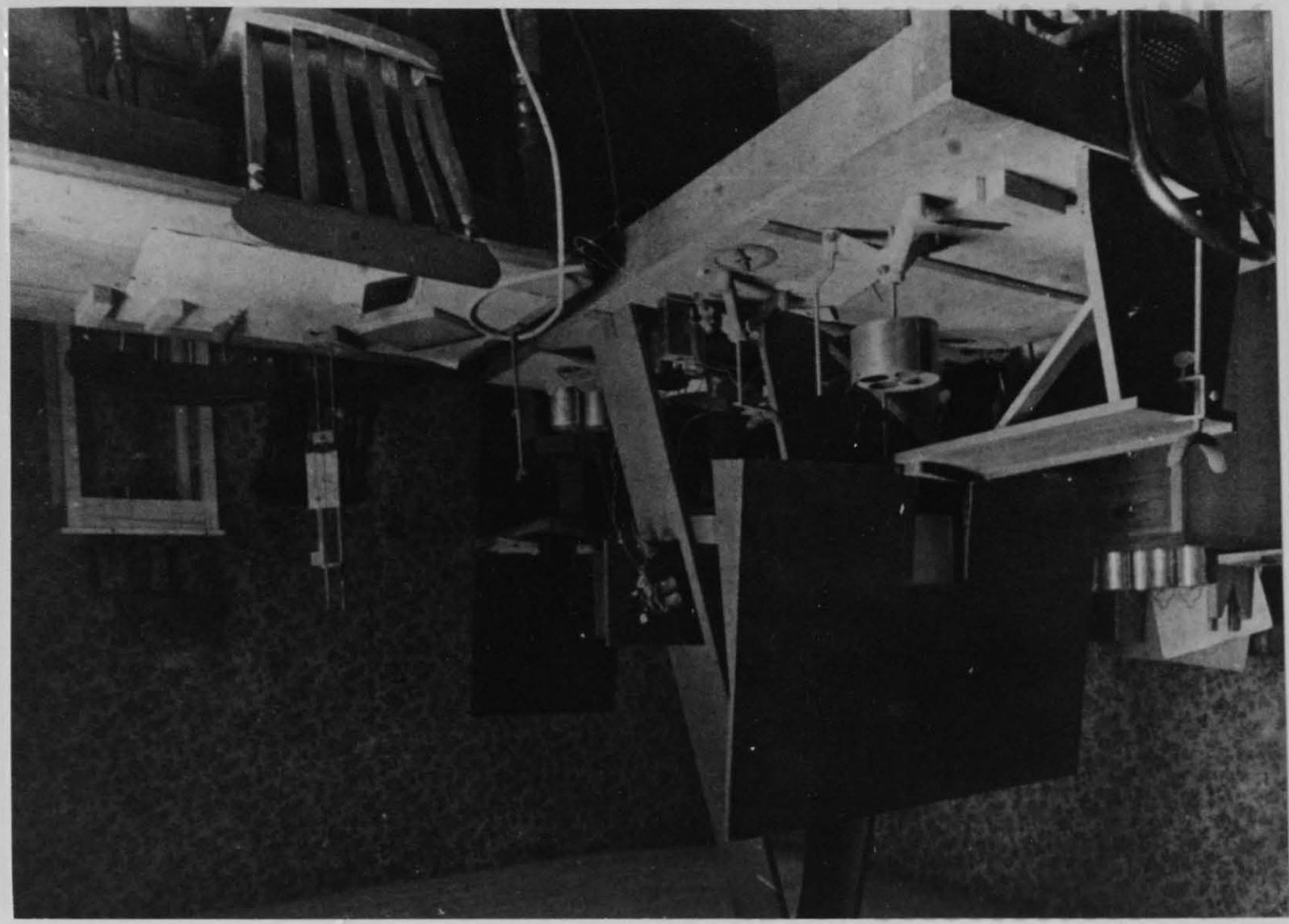
Plates 13 - 18 - appear in order on the following pages.

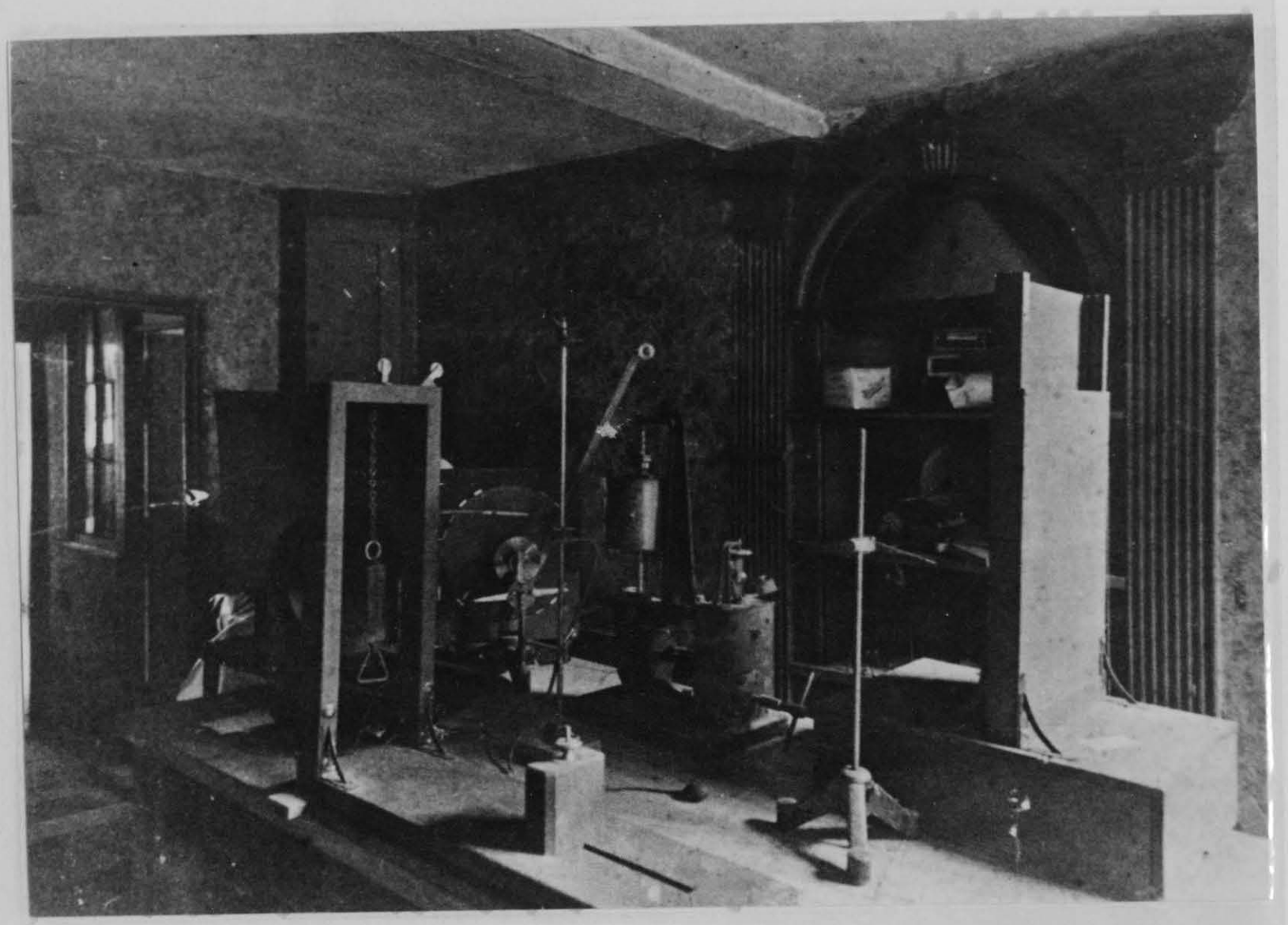
Plate 13 - A drawing of the new Physiology and Psychology laboratories, dated 1917.

Plate 14 - The outside of the Cambridge Laboratory's fourth home - 16 Mill Lane.

Plates 15,16,17,18 - Some apparatus from Mill Lane. Items include; a primitive "falling blade" tachistoscope; a flicker photometer - a primitive stroboscope; an early Eddison phonograph, with cylinders; a Stern variator - for producing tones of various pitches; a variable gearing device; an ergograph; several smoke drums - then used for measuring oscillations.


NEW BUILDINGS FOR THE UNIVERSITY OF CAMBRIDGE.
Physiological Laboratory (Gift of the Drapers' Company)
and Psychological Laboratory.
SEDGWICK MUSEUM of GEOLOGY.
SITE FOR EXTENSION
THEATRE
PHYSIOLOGY





APPENDIX 2

Notes on Archival Sources

A. Bartlett.

B. Myers.

C. Rivers.

D. Ward.

E. Cambridge University Library (C.U.L.)

F. Cambridge Psychological Laboratory (C.P.L.)

G. The N.I.I.P.

A. Bartlett

1. The C.U.L. holds a collection of Bartlett's unpublished papers. The collection was lodged at C.U.L. by the Contemporary Scientific Archives Centre (10 Keble Road, Oxford). The collection includes:

Return to Remembering - unpublished reflections on Remembering written around 1968.

What's the Use of Psychology - Chapter 1 only - in typescript.

Fellowship Dissertation - of 1916 in typescript.

Other C.U.L. papers include notebooks for 1917 - reporting some of the original Remembering experiments, lecture notes on Social Psychology and Stout's text books and lastly various drafts for (later published) articles.

2. Bartlett's executors hold the most interesting and complete collection of his papers. This includes most of his correspondence; course work; original drafts for papers and books; diaries kept on foreign trips.

Of most interest however is the typescript of Chapters 1, 2 and 3 of "What's the Use of Psychology" Bartlett's unpublished autobiography.

3. The C.P.L. holds a considerable amount of Bartlett's departmental correspondence especially from the years immediately before and after World War II.

4. Both St. John's College and C.P.L. hold useful collections of Bartlett's published papers.

B. Myers

1. Gonville and Caius College, the C.P.L. and the N.I.I.P. Library (at North East London Polytechnic) hold extensive collections of Myers' published papers and books.

2. The C.P.L. also holds a little departmental correspondence.

3. Myers' Torres Straits notebook is held in C.U.L.'s Haddon Collection.

4. The remainder of Myers' correspondence is held by the N.I.I.P., his executors and Miss Blunt his ex-secretary. Professor Alec Rodger holds the manuscript of "Ten More Years of Psychology".

C. Rivers

1. Rivers' voluminous notebooks, correspondence, article drafts and some lecture notes are held in C.U.L.'s Haddon Collection.

2. St. John's College and C.P.L. have good collections of his published work.

D. Ward

1. Trinity College holds a few letters from Ward from the 1870's and 1880's.

2. The C.P.L. holds what appear to be original manuscript copies of Ward's psychology lecture notes. These date from 1875 and appear to be the basis for much of his later work.

E. C.U.L. (See A, B and C above)

1. C.U.L. Archives hold minute books for:
Board of Moral Science 1860-1888.
Council of the Senate - the whole period of this thesis.
General Board of Studies - 1882 onwards.

2. They also hold notes on the development of:
Professorship of Mental Philosophy and Logic (CUR 39.42).
Reader (and Demonstrator) in Physiological and Experimental Psychology (CUR 113).

3. An extensive "Haddon Collection" - including much original data from the Torres Straits.

4. A complete "run" of the Cambridge University Reporter - a record of University events, appointments exams etc.

F. C.P.L. (See also A, B, C, D above)

1. Minutes book of the Psychological Board of Studies (1920 - 1926).

2. A collection of journal cuttings and specially written articles relating to the building of the laboratory.

3. Minutes book of the Psychological Laboratory Syndicate (1910 - 1913).

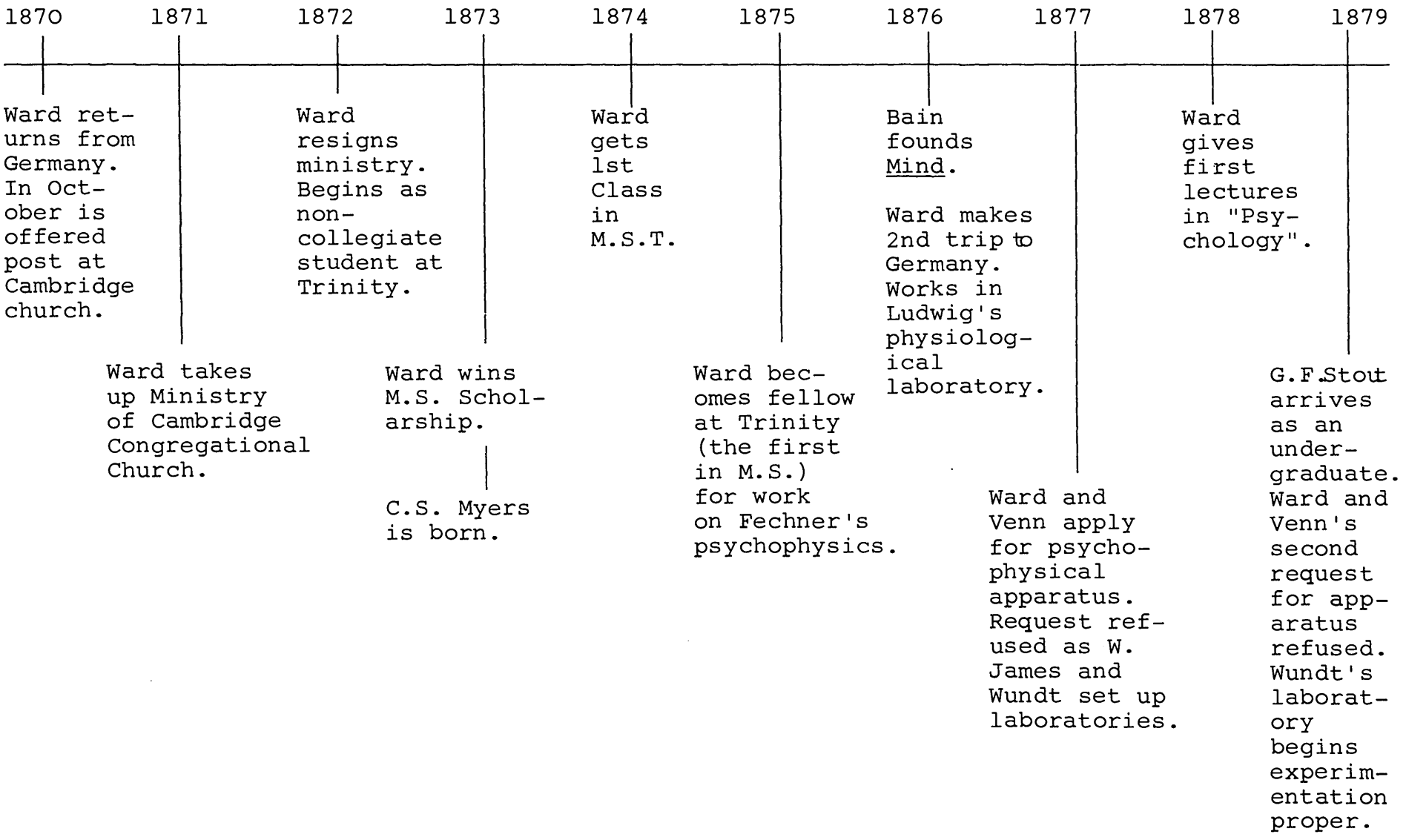
G. N.I.I.P.

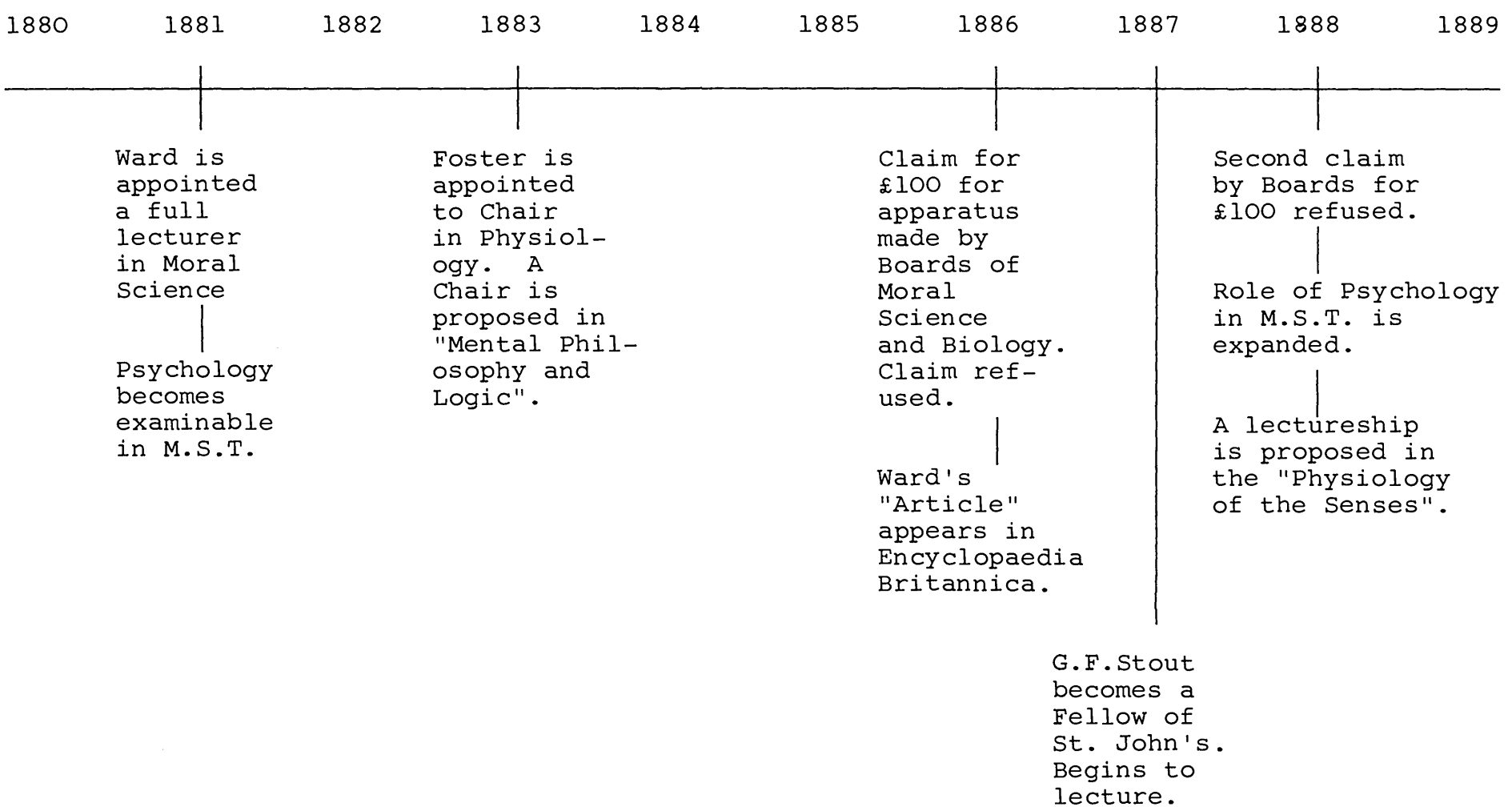
The N.I.I.P.'s library is now held at Livingstone House, C/o the North East London Polytechnic. The library holds an extensive collection of Myers' works. There is also; a useful collection of pamphlets, reports and advertising material from the 1920's and 1930's; a complete set of the N.I.I.P.'s journals; a host of other material far beyond the scope of this current research.

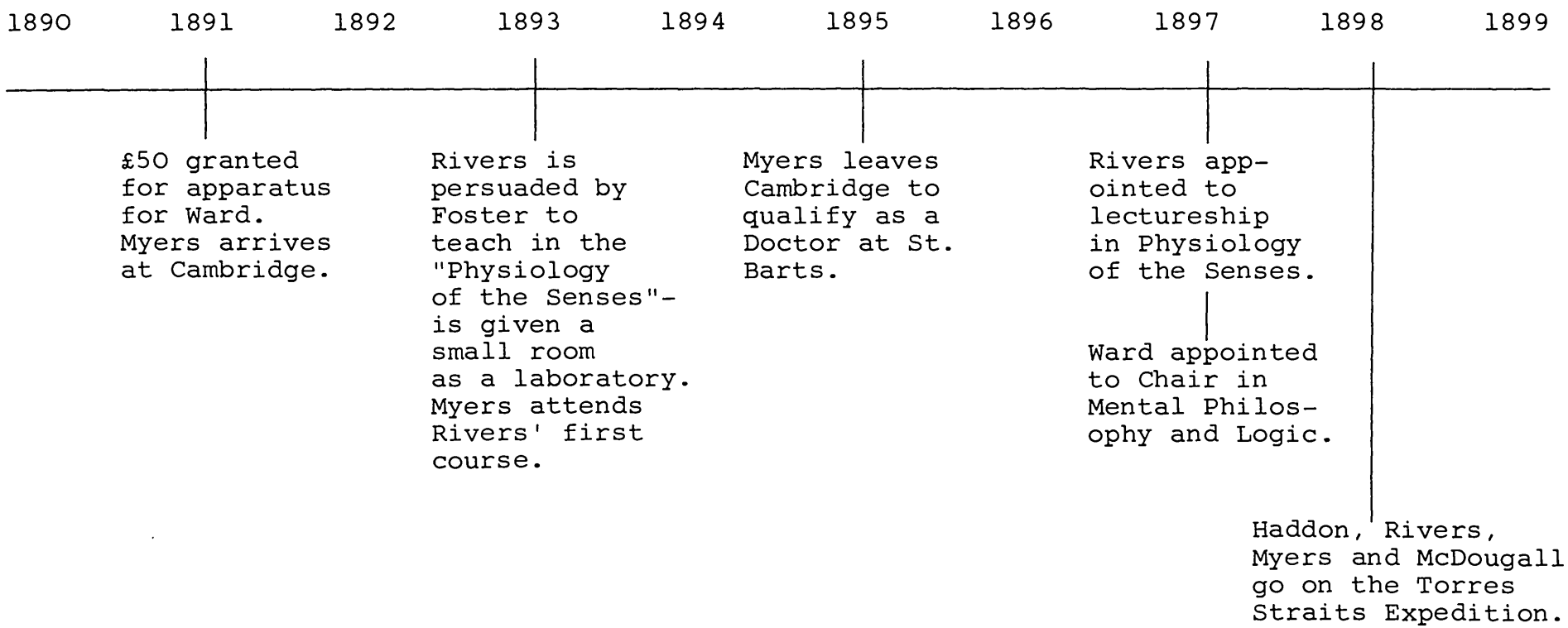
APPENDIX 3

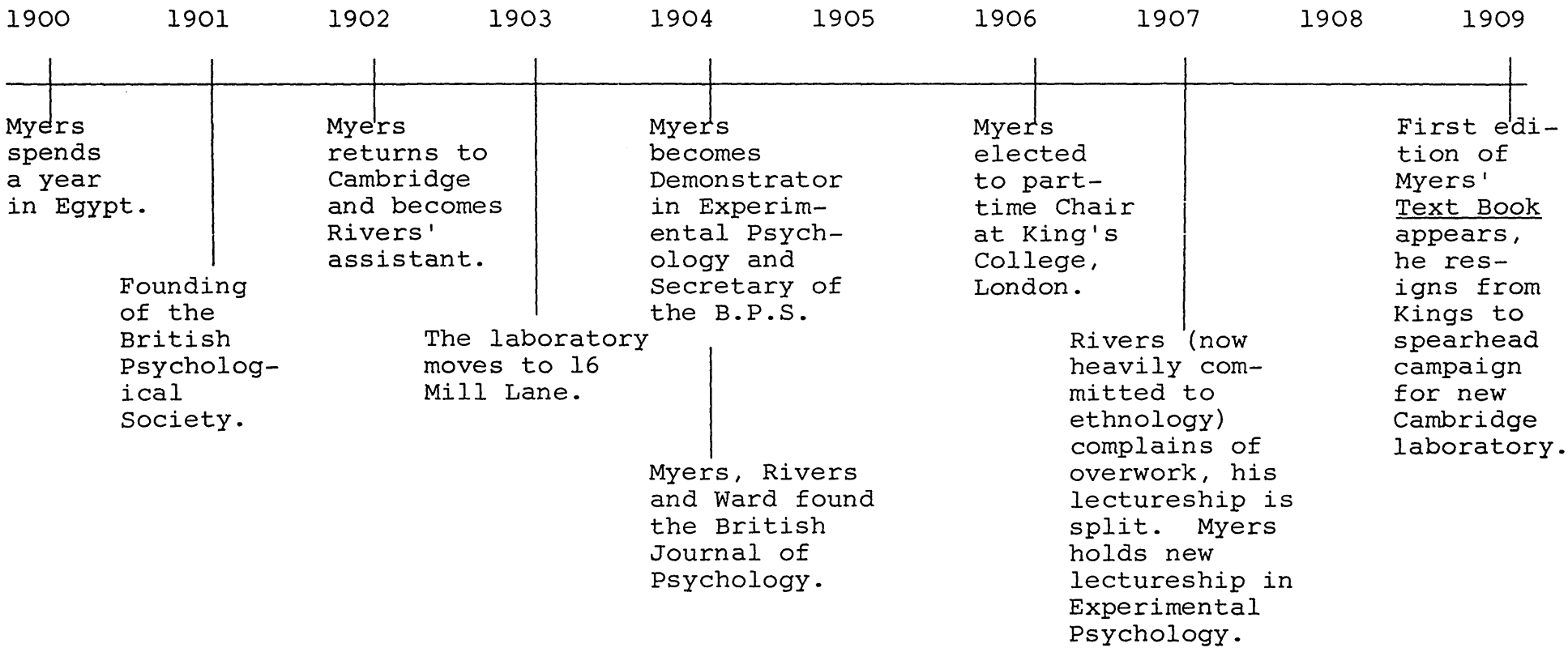
A "Time Line" for Cambridge Psychology

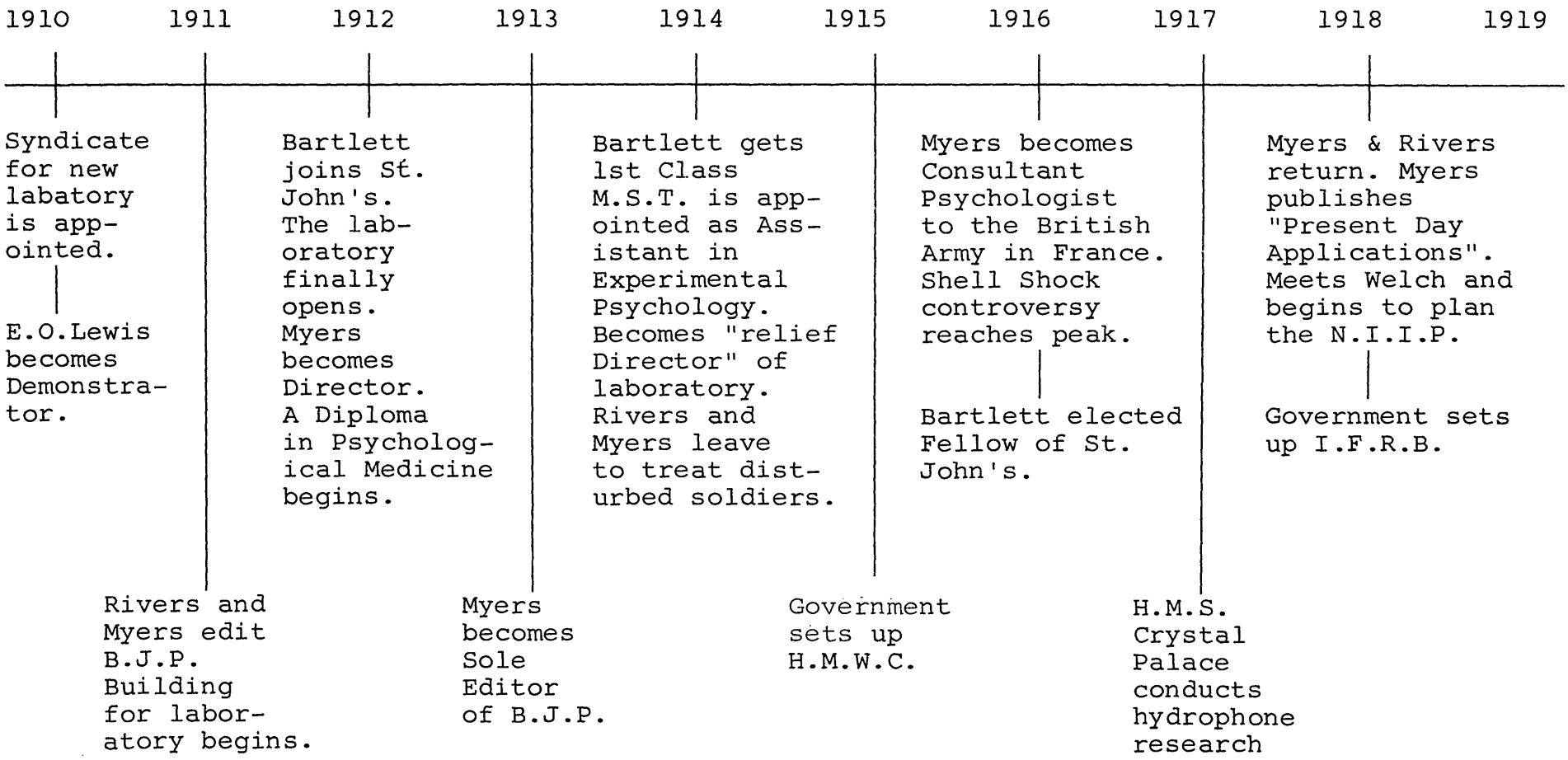
1870 - 1939

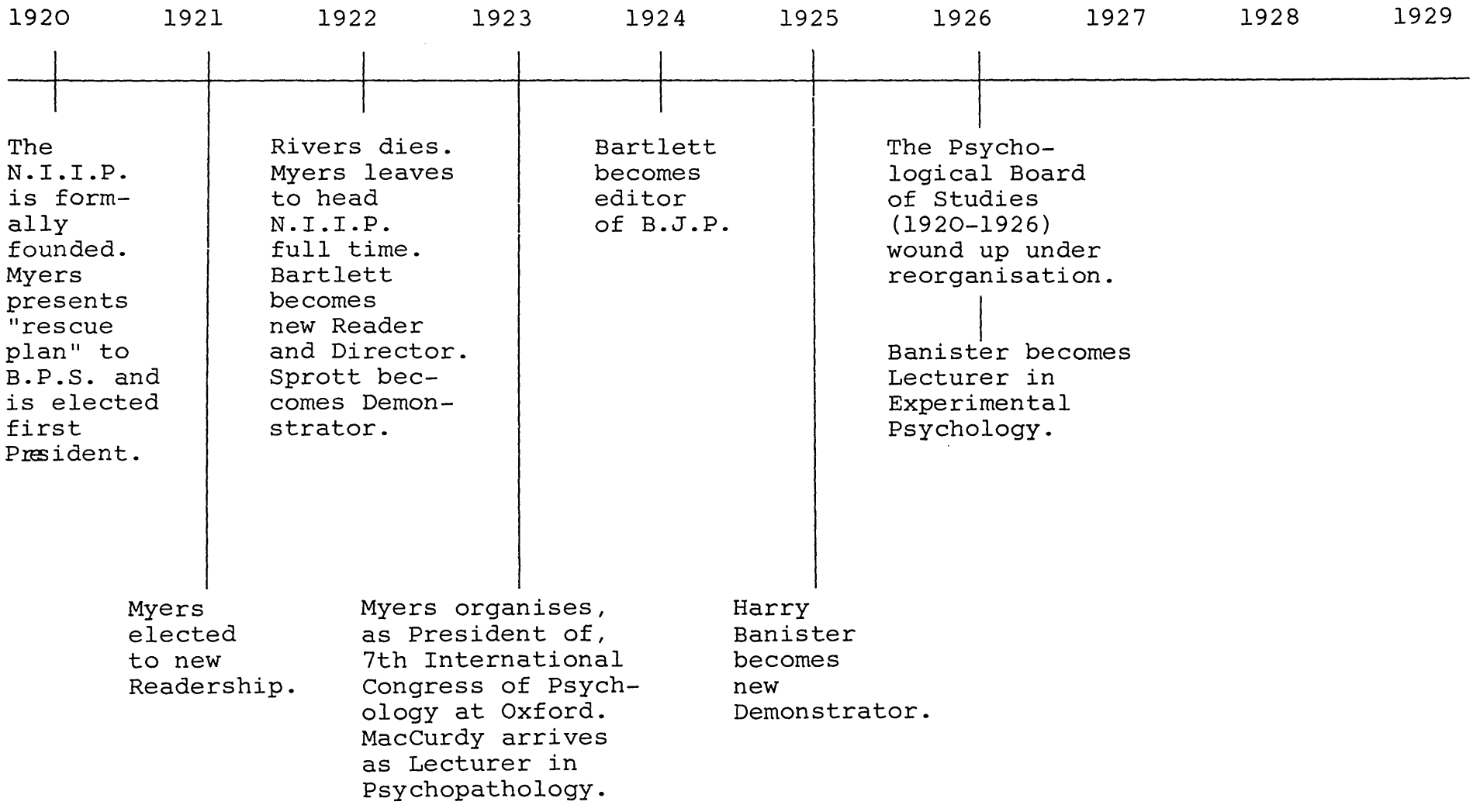

1870
1871
1872
1873
1874
1875
1876
1877
1878
1879
Ward returns from Germany. In October is offered post at Cambridge church.
Ward takes up Ministry of Cambridge Congregational Church.
Ward resigns ministry. Begins as non-collegiate student at Trinity.
Ward wins M.S. Scholarship.
C.S. Myers is born.
Ward gets 1st Class in M.S.T.
Ward becomes fellow at Trinity (the first in M.S.) for work on Fechner's psychophysics.
Bain founds Mind.
Ward makes 2nd trip to Germany. Works in Ludwig's physiological laboratory.
Ward and Venn apply for psychophysical apparatus. Request refused as W. James and Wundt set up laboratories.
Ward gives first lectures in "Psychology".
G.F. Stout arrives as an undergraduate. Ward and Venn's second request for apparatus refused. Wundt's laboratory begins experimentation proper.

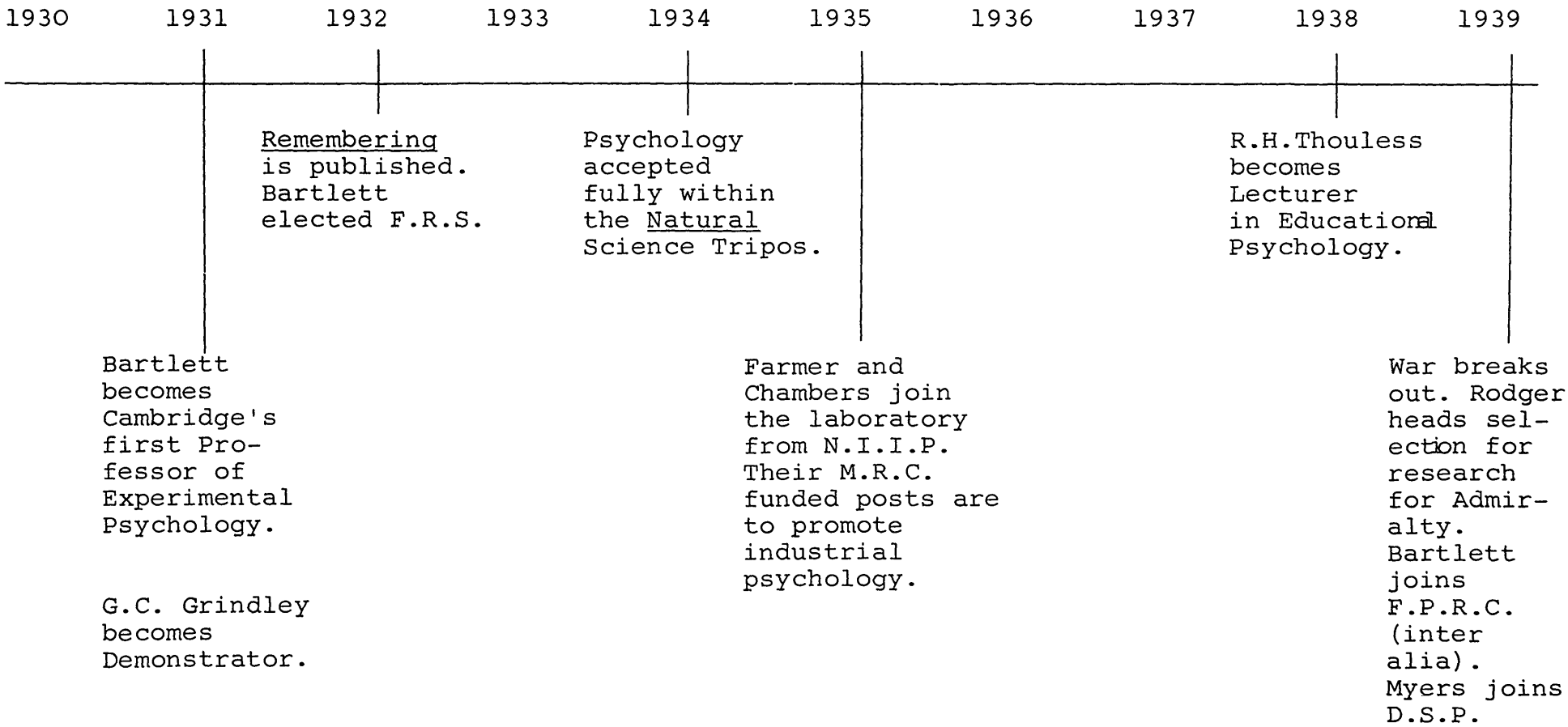

1880
1881
1882
1883
1884
1885
1886
1887
1888
1889
Ward is appointed a full lecturer in Moral Science
Psychology becomes examinable in M.S.T.
Foster is appointed to Chair in Physiology. A Chair is proposed in "Mental Philosophy and Logic".
Claim for £100 for apparatus made by Boards of Moral Science and Biology. Claim refused.
Ward's "Article" appears in Encyclopaedia Britannica.
G.F.Stout becomes a Fellow of St. John's. Begins to lecture.
Second claim by Boards for £100 refused.
Role of Psychology in M.S.T. is expanded.
A lectureship is proposed in the "Physiology of the Senses".


1890
1891
1892
1893
1894
1895
1896
1897
1898
1899
£50 granted for apparatus for Ward. Myers arrives at Cambridge.
Rivers is persuaded by Foster to teach in the "Physiology of the Senses"- is given a small room as a laboratory. Myers attends Rivers' first course.
Myers leaves Cambridge to qualify as a Doctor at St. Barts.
Rivers appointed to lectureship in Physiology of the Senses.
Ward appointed to Chair in Mental Philosophy and Logic.
Haddon, Rivers, Myers and McDougall go on the Torres Straits Expedition.


1900
1901
1902
1903
1904
1905
1906
1907
1908
1909
Myers spends a year in Egypt.
Founding of the British Psychological Society.
Myers returns to Cambridge and becomes Rivers' assistant.
The laboratory moves to 16 Mill Lane.
Myers becomes Demonstrator in Experimental Psychology and Secretary of the B.P.S.
Myers, Rivers and Ward found the British Journal of Psychology.
Myers elected to part-time Chair at King's College, London.
Rivers (now heavily committed to ethnology) complains of overwork, his lectureship is split. Myers holds new lectureship in Experimental Psychology.
First edition of Myers' Text Book appears, he resigns from Kings to spearhead campaign for new Cambridge laboratory.


1910
1911
1912
1913
1914
1915
1916
1917
1918
1919
Syndicate for new labatory is appointed.
E.O.Lewis becomes Demonstrator.
Rivers and Myers edit B.J.P. Building for laboratory begins.
Bartlett joins St. John's. The laboratory finally opens. Myers becomes Director. A Diploma in Psychological Medicine begins.
Myers becomes Sole Editor of B.J.P.
Bartlett gets 1st Class M.S.T. is appointed as Assistant in Experimental Psychology. Becomes "relief Director" of laboratory. Rivers and Myers leave to treat disturbed soldiers.
Government sets up H.M.W.C.
Myers becomes Consultant Psychologist to the British Army in France. Shell Shock controversy reaches peak.
Bartlett elected Fellow of St. John's.
H.M.S. Crystal Palace conducts hydrophone research
Myers & Rivers return. Myers publishes "Present Day Applications". Meets Welch and begins to plan the N.I.I.P.
Government sets up I.F.R.B.


1920
1921
1922
1923
1924
1925
1926
1927
1928
1929
The N.I.I.P. is formally founded. Myers presents "rescue plan" to B.P.S. and is elected first President.
Myers elected to new Readership.
Rivers dies. Myers leaves to head N.I.I.P. full time. Bartlett becomes new Reader and Director. Sprott becomes Demonstrator.
Myers organises, as President of, 7th International Congress of Psychology at Oxford. MacCurdy arrives as Lecturer in Psychopathology.
Bartlett becomes editor of B.J.P.
Harry Banister becomes new Demonstrator.
The Psychological Board of Studies (1920-1926) wound up under reorganisation.
Banister becomes Lecturer in Experimental Psychology.


1930
1931
1932
1933
1934
1935
1936
1937
1938
1939
Remembering is published. Bartlett elected F.R.S.
Psychology accepted fully within the Natural Science Tripos.
R.H.Thouless becomes Lecturer in Educational Psychology.
Bartlett becomes Cambridge's first Professor of Experimental Psychology.
G.C. Grindley becomes Demonstrator.
Farmer and Chambers join the laboratory from N.I.I.P. Their M.R.C. funded posts are to promote industrial psychology.
War breaks out. Rodger heads selection for research for Admiralty. Bartlett joins F.P.R.C. (inter alia). Myers joins D.S.P.

APPENDIX 4

Select bibliographies and biographical material.

A. F. C. Bartlett

B. C. S. Myers

C. W. H. R. Rivers

D. James Ward

(A) SIR F.C. BARTLETT

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